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HENRY FRANCIS CRONIN, C.B.E., M.C., B.Sc.(Eng.), Vice-President I.C.E., Chairman of the Division, in the Chair

The following Paper was presented for discussion and, on the motion of the Chairman, the thanks of the Division were accorded to the Author.

Public Health Paper No. 3

"Relation between Daily Rainfall and Flow of the River Shin"

by

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SYNOPSIS

The Author discusses a novel method of deriving the daily flow of the River Shin from the daily rainfall, based on consideration of the recession curve of daily flow at times of no rain. A very simple table has been devised to reduce the daily rainfall, first to what is termed the daily consumption of rainfall, and then by deduction of appropriate average daily evaporation loss, to give the daily flow. Hydrographs of the flow of the River Shin for the 3 years 1947, 1948, and 1949, calculated by the method described, have agreed with the hydrograph of actual observed flow. The method has been extended to calculate the daily flow of the River Shin for the years 1927 to 1946, over which period rainfall data, but no observed flows, are available. It is shown that the method is of general application if the value of the constants employed can be determined, and it explains several well-known features of river behaviour.

Introduction

This Paper describes what is thought to be a novel method, capable of general application, of correlating the daily rainfall and daily flow of the River Shin, devised from consideration of the recession curve of the flow during periods of no rain. It was found that the method could be made to give surprisingly good quantitative values for the relationship of daily rainfall to run-off for all stages of the flow—from flood peaks to dry spell minima, and also for conditions of both rising and falling river levels.

Calculation of the flow of the River Shin from the rainfall by the method set out in this Paper, for the years 1947 to 1949, agrees largely with the river flows actually observed, and it appears reasonable to suppose that a very fair estimate of the river behaviour can be made based on the daily rainfall data only. It should thus be possible to use the method to predict flood levels from a knowledge of the rainfall, and to reconstruct past river flows from earlier rainfall records.

Daily rainfall records have been kept in the catchment area of the River Shin for a considerable number of years, and in particular, from 1927 onwards, but no records of river flow prior to 1947 are available. The method has therefore been used to reconstruct the hydrograph of daily flow of the river for past years for which no discharges have been recorded. This is of use in the estimation of river flow for hydro-electric power development purposes, for which flow data for a long period are essential.

In many cases, in the British Isles at least, long-term flow records of rivers are lacking, while there are often ample rainfall records; and it is thought that the method here set out could be applied to any river for the catchment area of which sufficient rainfall and climatic data are available, if suitable adjustments are made in the value of the factors employed, but without further modification of the principle involved.

Before proceeding to explain how the daily rainfall can be converted into daily flow, the Author attaches short notes on the catchment area, and the rainfall and river-flow records, from which the arithmetical value of the factors applicable to the case of the River Shin have been derived.

Description of the River Shin Drainage Area

The River Shin, which lies wholly in the County of Sutherland in the north of Scotland, flows out of Loch Shin over a roughly made weir of about 90 feet in crest length. A discharge recording site was established on the river early in 1947, about 1 mile downstream of this weir.

The area drained by the river at the discharge recording site is 195 square miles. The area is practically treeless and there are numerous bogs. Loch Shin is at an elevation of 270 feet O.D., is 18 miles long and has a water surface of about 9 square miles. There are two additional lochs, of combined area of about 1½ square miles, connected with Loch

Shin by short lengths of river, and no part of the catchment is more than 8 miles from the central loch system. A map of the area is shown in *Fig. 1.*

The large storage capacity of the central loch system has the effect of reducing the range of discharge, by flattening the peaks of floods, and has thus facilitated this investigation into the relationship between daily rainfall and run-off.

Rainfall

The long-term average rainfall for the area is shown as isohyets in *Fig. 1*, and amounts to 52·60 inches per year.

The monthly and annual general rainfall is assessed from the monthly isohyetal maps, prepared by the Meteorological Office from the records of all the rain gauges lying both within and close to the area. The general daily rainfall cannot be assessed in this way, which would entail the preparation of daily isohyetal charts, if such were practicable.

Varying amounts of rain falling in different parts of the area have been assessed as if the rain fell evenly over the whole area, with suitable weighting of the records of individual rain gauges to account for the proportion of the catchment over which this influence is deemed to extend. This assessment of the rainfall seems to work well in practice, because at whichever end of the catchment the rain actually falls, it soon finds its way into the central loch system and thus makes its presence felt in about the same time.

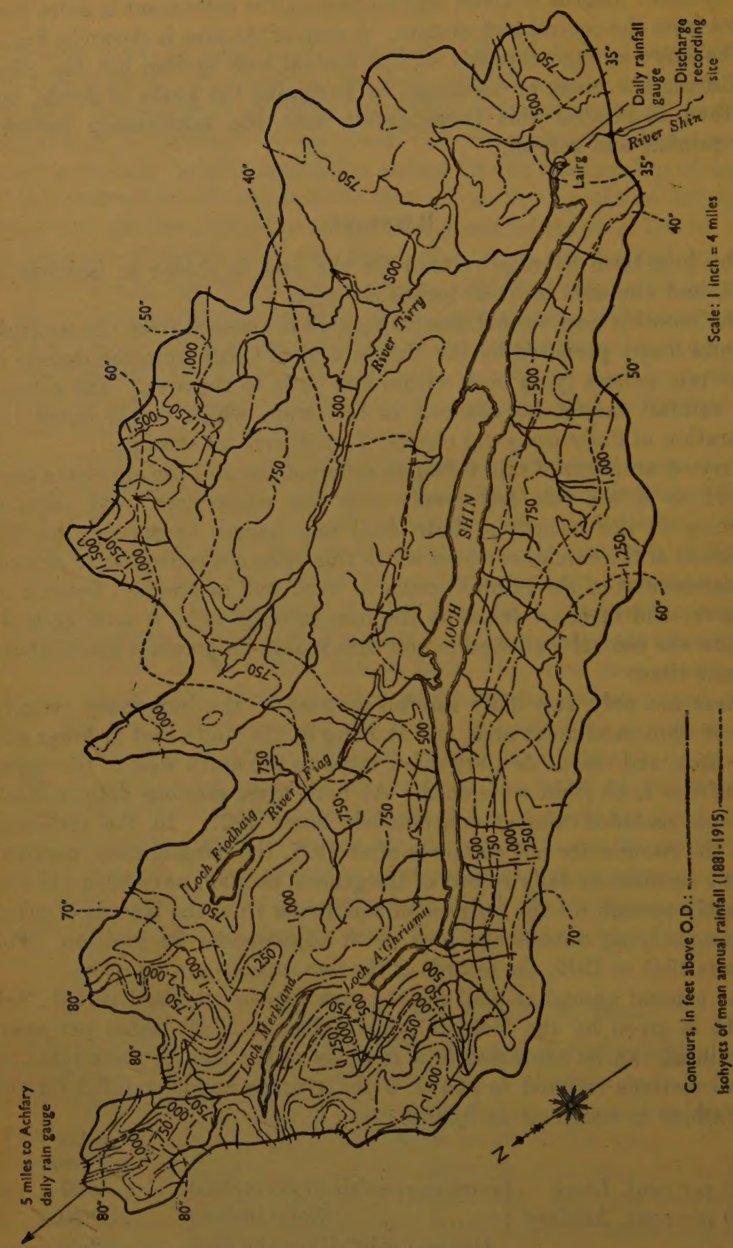
There are only two daily rainfall stations in the immediate vicinity of River Shin catchment area, one at Lairg at the south east or lower end of the area, and one at Achfary a few miles to the north west of the upper limit of the Loch Shin watershed. At both these stations daily rainfall has been recorded almost continuously since 1927. In the instances where the records are not complete after 1927, the missing data, required to give a continuous daily record of the general rainfall, have been filled in accurately enough for the purpose of this Paper from the records of other rain gauges lying outside the area, with adjustments for position. For the years 1947 to 1949, the records of the two stations are complete.

The general annual rainfall for the area for the standard period 1881 to 1915 is given by the Meteorological Office as 52·60 inches per year. This rainfall can be expressed as a function of the sum of the rainfall at the two stations referred to above, if the average annual rainfall for the two stations is weighted as follows :—

		inches per year
60 per cent. Lairg	(average rainfall 37·30 inches) . .	22·40
40 per cent. Achfary	(,, ,, 75·60 inches) . .	30·20
	General rainfall for the area . .	52·60

This method of weighting rainfall records to estimate the general

Fig. 1



RIVER SHIN CATCHMENT AREA

rainfall is adopted by Captain W. N. McClean [1] in his studies of another Scottish area.

The monthly assessment of the general rainfal for 1947 to 1949 made on the basis of 60 per cent. Lairg to 40 per cent. Achfary, and the monthly assessment of the general rainfall made by the Meteorological Office from the isohyetal charts, are set out in Table 1. It will be seen that the annual

Table 1.—COMPARATIVE METHODS OF ASSESSMENT OF THE MONTHLY GENERAL RAINFALL: RIVER SHIN ABOVE LAIRG

	1947		1948		1949	
	60% Lairg plus 40% Achfary	Meteorological Office monthly total	60% Lairg plus 40% Achfary	Meteorological Office monthly total	60% Lairg plus 40% Achfary	Meteorological Office monthly total
January . .	3·43	3·50	7·31	7·00	11·07	10·00
February .	1·40	1·75	4·34	4·60	6·43	5·70
March . .	2·63	2·70	4·51	5·00	4·51	4·50
April . .	5·83	5·60	4·39	3·70	5·16	4·90
May . . .	2·14	2·30	2·14	2·30	3·12	3·10
June . . .	3·43	3·60	3·26	3·20	1·85	1·90
July . . .	3·24	3·20	3·73	3·70	1·92	1·90
August . .	0·24	0·25	4·46	4·50	5·24	5·50
September .	8·99	8·80	6·83	6·90	3·99	3·90
October . .	3·81	3·80	6·43	7·00	4·63	4·40
November .	8·93	8·80	5·94	6·00	4·48	5·00
December .	5·09	4·60	5·54	6·20	13·53	11·50
Total . .	49·16	48·90	58·88	59·10	65·62	62·30

totals of the rainfall, obtained by the weighting method from two rainfall stations, are in fair agreement with the annual total given for the same area by the Meteorological Office, and that the monthly totals are also in reasonably consistent agreement over the whole period.

The general daily rainfall for the area, in so far as it has a bearing on river flow for the purposes of this Paper, has been deduced by weighting

[1] The references are given on p. 38.

the daily records of the above two rainfall stations in the same proportion as has been done for the monthly and annual rainfall.

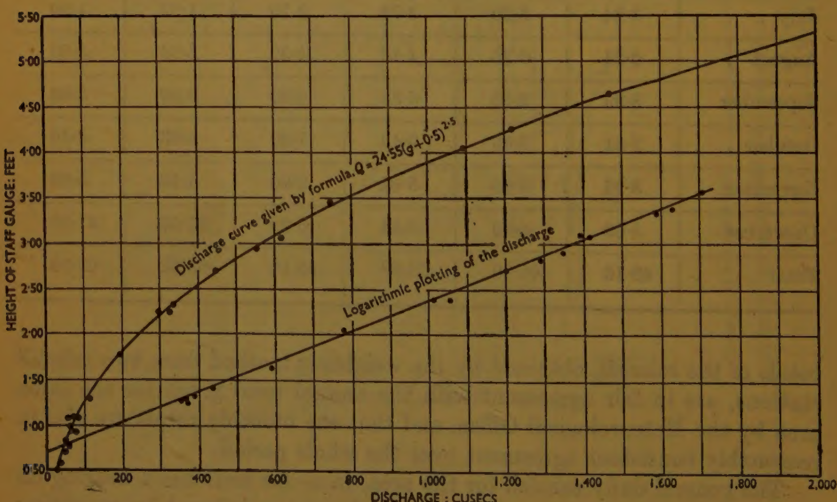
It has been accepted that in order to determine the general rainfall for any area correctly it is necessary to have one rainfall gauging station for every 2,000 acres.[2] This requirement would entail the establishment of about fifty gauges for the River Shin catchment, a requirement that could never be attained.

Although the rainfall stations taken for this study are clearly not enough to deduce the daily rainfall with complete certainty, yet the use of the records of the two rainfall stations, weighted as explained above, appears to give reasonably consistent results in the light of the relationship that has been found to exist between the rainfall and the observed run-off. It has the advantage that it is easy to use ; all that is required is the record of the daily rainfall, from which the assumed general daily rainfall is then easily and quickly derived arithmetically without further complication.

Daily Record of Flow of the River Shin

A discharge recording site was established on the river early in 1947, and a stage discharge curve shown in *Fig. 2* was determined from the result of numerous discharge observations by current meter taken over a fair range of river levels. An automatic level recorder has been installed so that a more reliable estimate of the daily flow, especially at

Fig. 2



River Shin : Stage Discharge Curve (observed discharges taken by current-meter between 27/2/47 and 16/5/47) for the Discharge Recording Site

peak periods, will be possible in future. In the meantime the daily flow is inferred from the mean of the river level read visually every morning, and the level read after the ensuing 24 hours. This brings the record of the flow into line with the practice followed, in recording rainfall, by the Meteorological Office, which gives the total depth of rain in inches on the day listed but which is actually measured on the subsequent morning. Extremes in change of river level from day to day only occur at rises to peak discharges, and in general the daily rises and falls are only a few inches, and it is assumed for the purpose of this study that the daily flow can be closely enough estimated for the present by daily visual readings of the gauge height.

The mean flow of the river as abserved in the 3 years 1947 to 1949 is at the rate of 600 cusecs. The minimum so far recorded being 27 cusecs and the maximum 3,200 cusecs. As calculated from the rainfall by the method set out in this Paper, the maximum during the period 1927 to 1947 was 3,770 cusecs.

From evidence of high water marks reached by the river in the last century, it is thought that the highest discharge in the past 100 years has been 8,000 to 9,000 cusecs. This is in accordance with the flow to be expected from the catchment area by the criterion laid down in the Interim Report of the Institution Committee on Floods.[3]

Recession Curve of Daily Flow at Time of No Rain

The observed daily flow of the River Shin shows that when no rain falls the flow decreases steadily, the drop in flow becoming less and less at a regular rate. The slope of the recession curve is seen to depend on its height, thus the higher the rate of flow the greater the recession and vice versa. Every period of no rain exhibits a similar recession curve of flow, and in general the slope of the curve is similar for similar rates of flow. This steady reduction in daily flow on occasions of no rain may be explained as follows :—

Assume that a certain quantity of rain has fallen : consider this rain, once it is on the ground as at first stored, whether on the ground, in the subsoil, or in lochs, pools, streams, etc. (that is, valley storage).

This stored rainfall will immediately begin to decrease by run-off and evaporation. If the factor governing the run-off is in the first instance assumed to be a function of the available head, then as the flow decreases the head will also decrease, and at fixed intervals of time, such as one day to the next, the amount remaining in storage will be a definite proportion of the previously stored volume. The amount lost to storage each day is, of course, equal to the daily flow plus any evaporation, and this combined daily loss in storage is termed by the Author, for want of a better word, the daily consumption of the rainfall.

The curve of consumption is considered as following approximately the recession in daily flow, the difference, if any, being due to the daily loss by evaporation, in which transpiration by plants is deemed to be included. The recession curves of consumption and storage although naturally complex and, as will be shown later, logarithmic in form, may be set out very simply in the form of a converging series as follows :—

Let the number of inches of rain already fallen be R.
Each day the quantity R is reduced by run-off, etc.
Let the diminishing factor from day to day be K.
Let the rate on any given day of combined run-off and evaporation be QE : this has been termed by the Author the daily consumption of rainfall.

Since the factor K is assumed here to be a function of the available head (or water held in storage), it may be assumed that K will remain the same for recurrences of the same storage levels. To simplify the set out of the converging series, let it be assumed in the first instance that the factor K is constant.

The curve of recession in storage, at the time, of no subsequent rain, is shown diagrammatically in *Fig. 3*. It can easily be seen that if the storage decreases daily by the factor K, then the consumption will also decrease by the same factor K.

Thus if the storage on successive days is R, KR, K^2R, K^3R, K^nR, then the consumption on the first day, QE_1, will be

$$R - KR = R(1 - K) \quad . \; . \; . \; . \; . \; . \quad (1)$$

and the consumption on the second day, QE_2,

$$KR - K^2R = KR(1 - K) \quad . \; . \; . \; . \; . \; . \quad (2)$$

then

$$\frac{QE_2}{QE_1} = \frac{KR(1 - K)}{R(1 - K)} = K \quad . \; . \; . \; . \; . \; . \quad (3)$$

hence

$$QE_2 = KQE_1 \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (4)$$

That is, the consumption on any given day is equal to the consumption on the previous day multiplied by K.

From *Fig. 3* can be seen that the consumption on any given day, n, after commencement of the series is :—

$$K^{n-1}R - K^nR = K^{n-1}R(1 - K) \quad . \; . \; . \; . \; . \quad (5)$$

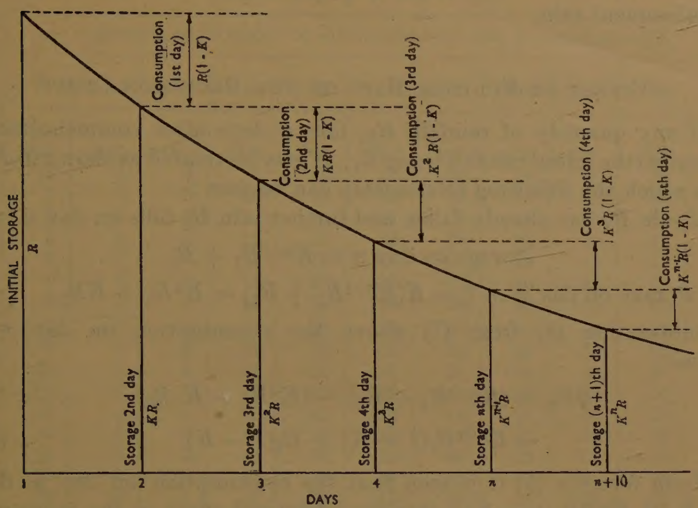
and the total rainfall R can be accounted for by the converging series

$$R = R(1 - K) + KR(1 - K) + K^2R(1 - K) \ldots . K^{n-1}R(1 - K) \quad (6)$$

in which each term of the series represents one day's consumption and is merely the previous term multiplied by K.

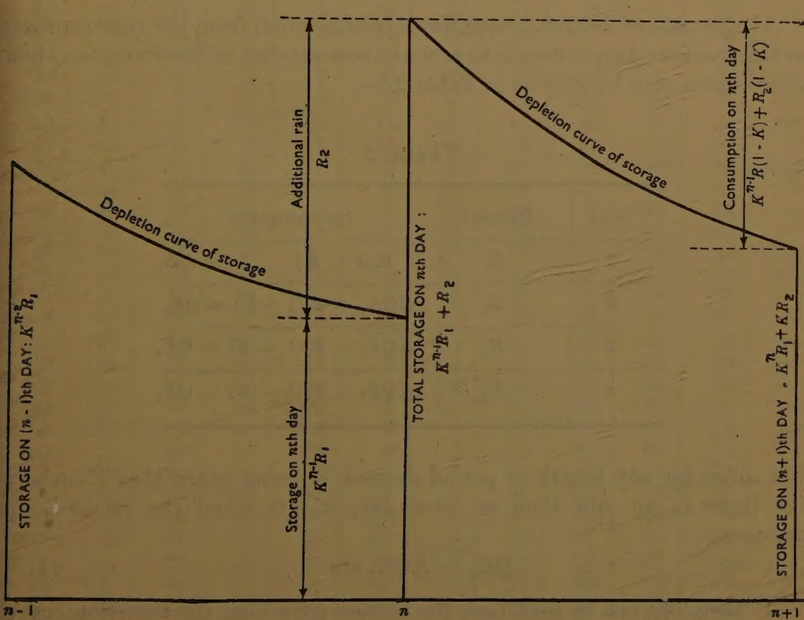
As K is fractional, after a number of days the consumption will

Fig. 3



Depletion Curve of Storage and River Flow at Time of No Rain

Fig. 4



Depletion Curve of Storage and River Flow at Time of Further Rain

become very small and finally negligible unless the storage is replenished by subsequent rain.

Effect of Further Rain on the Recession Curve

If any quantity of rainfall, R_2, falls n days after commencement of the series (the initial rainfall being R_1), it may be treated as shown in *Fig. 4*, from which the following relationship can be seen :—

If rain R_1 has already fallen and further rain R_2 falls on day n;

$$\text{storage on day } n = K^{n-1}R_1 + R_2 \quad . \quad . \quad . \quad . \quad (7)$$

$$\text{and storage on day } n+1 = K(K^{n-1}R_1 + R_2) = K^nR_1 + KR_2 \quad . \quad (8)$$

Subtracting (8) from (7) above the consumption on day n will become

$$QE_n = (K^{n-1}R_1 + R_2) - (K^nR_1 + K.R_2) \quad . \quad . \quad . \quad (9)$$

$$= K^{n-1}R_1(1-K) + R_2(1-K) \quad . \quad . \quad . \quad . \quad (10)$$

From formula (5) it is seen that the consumption on day n, due to rain R_1, is $K^{n-1}R_1(1 - K)$: thus the additional effect on the consumption on day n due to R_2 is to add $R_2(1 - K)$ to the series, and the general equation for computation of the consumption from that of the prior day may be written thus,

$$QE_2 = KQE_1 + R(1-K) \quad . \quad . \quad . \quad . \quad (11)$$

The process of deducing each day's consumption from the consumption on the previous day, taking into account new rainfall as it occurs, can then be set down step by step as in Table 2 :—

Table 2

Day	Rainfall	Consumption
1	R_1	$R_1(1-K) \qquad = QE_1$
2	R_2	$KQE_1 + R_2(1-K) = QE_2$
3	R_3	$KQE_2 + R_3(1-K) = QE_3$
4	R_4	$KQE_3 + R_4(1-K) = QE_4$

and so on for any length of period desired, it being noted that if on any day there is no rain then on that day $R = 0$, when the relationship becomes :—

$$QE_2 = KQE_1 \text{ etc.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It therefore can be seen that the factors governing the recession curve of daily consumption at times of no rain (see pp. 7–10), can easily be

TABLE 3.—CONSUMPTION ON THE RIVER SHIN: SOLUTION OF THE EQUATION

$$QE_2 = KQE_1 + R(1 - K)$$

QE_1	Amount to be added or subtracted from QE_1 to determine QE_2										
	$R = 0$	0·10	0·20	0·30	0·40	0·50	0·60	0·70	0·80	0·90	1·00
	—	+									
·030	·001	·002	·003	·006	·009	·012	·015	·018	·022	·026	·029
40	1	2	4	8	12	14	20	24	28	32	36
50	1	2	5	10	14	18	23	27	32	38	43
60	2	2	6	11	16	21	26	31	37	44	50
70	3	1	6	12	17	23	28	34	40	48	55
80	4	1	7	13	19	25	30	37	44	52	59
90	5	1	7	13	20	26	33	40	48	56	64
·100	6	0	7	13	20	28	35	43	51	60	68
10	7	1	6	13	21	29	37	45	54	63	72
20	9	2	6	14	22	31	38	46	56	66	75
30	10	2	6	14	22	31	39	48	58	68	78
40	11	3	5	13	22	32	41	50	59	70	81
50	12	4	4	13	22	33	41	51	60	72	83
60	14	5	4	13	23	33	42	52	62	74	85
70	15	6	3	13	23	33	42	53	64	76	88
80	17	8	2	12	22	33	43	54	65	78	90
90	19	9	1	12	22	33	43	55	66	80	92
·200	20	10	0	11	22	33	44	56	68	81	94
10	22	11	1	10	21	33	44	56	68	82	94
20	24	13	2	9	21	32	44	56	69	83	95
30	25	14	3	8	20	32	44	56	69	84	96
40	27	16	5	7	19	31	43	57	70	85	97
50	29	17	6	6	18	31	43	57	70	85	98
60	31	19	7	5	18	30	43	57	70	85	99
70	33	21	9	4	17	30	42	57	70	85	·100
80	35	23	10	3	16	29	42	56	70	85	101
90	37	24	11	2	15	28	42	56	70	85	101
·300	39	26	13	0	14	27	41	56	70	86	102
10	41	28	15	2	12	27	41	55	70	86	102
20	43	30	16	3	11	26	40	55	69	85	102
30	45	32	18	4	10	25	40	54	69	85	102
40	47	34	20	6	9	24	39	53	69	84	103
50	49	36	22	7	7	23	38	52	68	83	103
60	51	38	23	9	6	22	37	51	67	83	103
70	53	40	25	10	5	20	35	50	66	82	103
80	55	42	27	12	3	19	34	49	65	82	103
90	57	44	28	14	2	18	33	48	64	81	104
·400	60	46	30	15	0	16	32	47	63	80	104
10	62	48	32	17	2	15	31	46	63	80	103
20	64	50	34	18	3	13	30	46	62	79	102
30	67	52	36	20	4	12	28	45	62	79	101
40	69	54	38	22	6	10	27	44	61	78	·100
50	72	57	40	24	8	9	26	43	60	78	100
60	74	59	42	26	9	7	24	42	60	77	99
70	77	61	45	28	11	5	23	41	59	77	98
80	79	64	47	30	13	3	21	39	58	76	97
90	82	66	49	32	15	2	20	38	57	76	96
·500	85	69	52	35	18	0	18	37	56	76	96
10	88	72	55	37	20	2	16	35	55	75	95
20	91	74	57	40	22	4	15	34	53	73	94
30	93	77	60	42	24	5	13	32	52	72	92
40	96	79	62	45	26	7	12	31	50	70	91
50	99	82	65	47	28	9	10	29	49	69	90

adapted to times of continued rain without any further change in the principle involved.

Computation of Consumption from Daily Rainfall

If an estimate can be made of the value of the factor K, a table of consumption of daily rainfall can be composed, step by step from one day to the next, from the daily rainfall records based on the foregoing formulae.

A table enabling the consumption on any day to be read off directly from a combination of the rainfall and the consumption on the prior day, worked out beforehand for all relevant values of the consumption, is given in Table 3, the method of calculating the Table being given on pp. 13–19. In preparation of the above Table, K has been assigned a sliding value as explained on pp. 13–19.

The effect of subsequent rain on the converging series given in formula (6) may also be written out in full as in Table 4.

Table 4

Day	Rain	Consumption, QE				
		1st day	2nd day	3rd day	4th day	etc.
1	R_1	$R_1(1-K)+$	$KR_1(1-K)+$	$K^2R_1(1-K)+$	$K^3R_1(1-K)$	etc.
2	R_2		$R_2(1-K)$	$+KR_2(1-K)+$	$K^2R_2(1-K)$	etc.
3	R_3			$R_3(1-K)$	$+KR_3(1-K)$	etc.
4	R_4				$R_4(1-K)$	etc.

The consumption on each day can be obtained by summation of each of the above terms for its appropriate day. For instance, the consumption on day 2 is, $KR_1(1-K)+R_2(1-K)$, which is in the same form as that given in formula (10) for the second day's consumption.

From the set-up of the above form for the total consumption on any one day, it will be seen that the effect of any day's rain diminishes steadily in time, irrespective of the effect of subsequent rain, and it is found that the effect of any specific day of rain will become negligible in about 2 months.

It follows from the above that any arithmetical errors in the rainfall data, or other initial mistake, will not materially affect the calculation of the consumption for later dates in spite of the fact that the process of the calculation is a continuous one, step by step from one day to the next.

In starting to work out the consumption for any period, the rain which

fell in the immediately preceding period is the factor which determines the amount of the consumption on the selected starting day. An approximate estimate of the influence of prior rain can be made by assuming a fixed mean value for K, and by treating each day separately, the influence of a previous day's rain being gauged by counting the number of days between its fall and the selected starting date. If the number of days is n, then the influence of that day on the starting date is given by the term, $K^{n-1}R(1 - K)$, and so on for each other day. The sum of all the influences is then taken as the value of QE on the starting date.

After settling the value of QE on the starting date, it is not necessary to treat subsequent days of rain by the laborious process of separate summation of all the relevant terms in the diminishing series set out above, and the operation can be carried out step by step by the process explained on pp. 10–12.

It will be seen from the arrangement of the diminishing series given above, and earlier, that the assigning of a variable value to K (as long as K remains fractional) will only affect the complexity of the formula, and will not actually alter its form, or the ultimate convergence of its final terms to zero, or the fact that the total sum of the series must equal the total rainfall.

Value to be Adopted for the Factor K

An approximate hydrograph of the daily consumption can be deduced from the daily rainfall by adopting a fixed value of 0·90 for K for the River Shin; but this hydrograph does not follow that of observed flow as closely as is possible if a variable value of K is assumed, with K approaching unity as the flow approaches zero; because as the discharge becomes low the recession curve of flow becomes progressively flatter than can be accounted for by taking the value of 0·90 for K to be constant. Similarly, at times of recession of the flow after very high peaks, K requires to be 0·75 in order to make the calculated and observed curves of equal slope. This progressive flattening of the flow curve at low values has already been noted as a feature of river flow.[4]

As will be shown later, the factor K in the case of the steady emptying of a reservoir over a fixed weir can easily be found mathematically, and the curve of its value is seen to follow a progressive flattening similar to that of the curve of empirical values assigned to K for the natural catchment area of the river. The factor K for the River Shin has therefore been assigned a sliding value dependent on the level of the consumption and is shown plotted in the form of a smooth curve in *Fig. 5*, which also shows the corresponding curve for the factor K for the Loch Shin weir derived by calculation referred to on pp. 33–34.

On the above basis the recession curve of daily consumption at times of no rain will be logarithmic in form. The logarithmic nature of the

TABLE 5.—MONTHLY EVAPORATION LOSSES DERIVED FROM RAINFALL LESS OBSERVED FLOW AND FROM CONSUMPTION LESS OBSERVED FLOW

	1947					1948					1949				
	(a) General rainfall	(b) Observed flow	(c) Loss (Col. a–b)	(d) Consumption	(e) Evaporation (Col. d–b)	(f) General rainfall	(g) Observed flow	(h) Loss (Col. f–g)	(k) Consumption	(l) Evaporation (Col. k–g)	(m) General rainfall	(n) Observed flow	(o) Loss (Col. m–n)	(p) Consumption	(q) Evaporation (Col. p–n)
January .	3·43	3·62	−0·19	4·59	0·97	7·21	6·16	1·15	7·07	0·91	11·07	11·04	0·03	10·92	−0·12
February .	1·40	0·62	0·78	1·95	1·33	4·34	5·82	−1·45	6·11	0·29	6·43	3·93	2·50	4·89	0·96
March . .	2·63	1·69	0·94	2·33	0·64	4·51	3·30	1·21	3·94	0·64	4·51	6·03	−1·52	7·04	1·01
April . .	5·83	4·99	0·84	3·82	−1·17	4·39	2·63	2·13	4·42	1·79	5·16	3·26	1·90	4·38	1·12
May . .	2·14	2·67	−0·53	4·37	1·70	2·14	1·29	0·85	2·93	1·64	3·12	1·76	1·36	3·88	2·12
June . .	3·43	1·39	2·04	3·18	1·79	3·26	0·87	2·39	2·95	2·08	1·85	0·85	1·00	2·68	1·83
July . .	3·24	1·20	2·04	3·54	2·34	3·73	2·08	1·65	3·71	1·63	1·92	0·32	1·60	1·76	1·44
August . .	0·24	0·37	−0·13	1·65	1·28	4·46	2·48	1·98	3·74	1·26	5·24	1·84	3·40	4·83	2·99
September .	8·99	2·25	6·74	5·22	2·97	6·83	4·67	2·16	6·50	1·83	3·99	1·87	2·12	3·89	2·02
October .	3·81	3·94	−0·13	6·01	2·07	6·43	5·82	0·61	6·98	1·16	4·63	2·21	2·42	4·18	1·97
November .	8·93	6·76	2·17	7·47	0·71	5·94	5·99	−0·05	6·73	0·74	4·48	3·00	1·48	4·68	1·68
December .	5·09	4·09	1·00	5·16	1·07	5·54	3·59	1·95	4·58	0·99	13·52	11·55	1·97	12·37	0·82
Total . .	49·16	33·59	15·57	49·29	15·70	58·88	44·33	14·55	59·66	15·33	65·92	47·66	18·26	65·50	17·84

recession curve of daily flow has been noted by Meyer who has also shown that storage and flow-off are directly proportional.[5]

This increasing value of K towards unity is also in accordance with the well-known fact that, at low stages of river flow, rain has much less effect than at higher discharges, when the ground is presumed to be saturated. The additional consumption due to rain R_2 is given by the term $R_2(1 - K)$, and at low discharge this term becomes very small however heavy the rain, since then the term $(1 - K)$ approaches zero.

The curve of values for K has been determined empirically for the River Shin, by trial and error, to make the consumption curve run as close as possible to the hydrograph of flow so far observed. Further research may indicate better values, for although the curve of K has been assumed to be a smooth curve, it is supposed to include the daily reduction due to evaporation losses which may well vary in a way quite unrelated to the clearly smooth logarithmic curve of actual recession of flow if there were no rain and no evaporation. Further, it cannot be strictly correct to assume that the curve of K should pass through unity at zero consumption, since that entails the paradox that at time of no consumption no amount of subsequent rain would produce any effect.

From a perusal of Table 3, and as seen from the equations given on pp. 10–12, the operation of computing the consumption resolves itself in successive determinations of the equation,

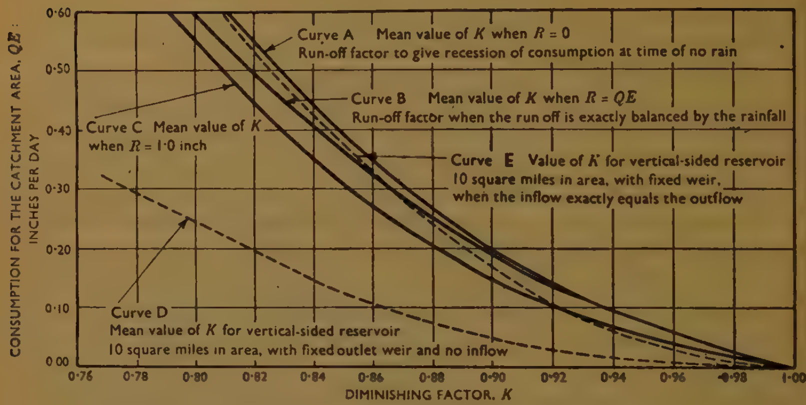
$$QE_2 = KQE_1 + R(1 - K) \qquad . \quad . \quad . \quad . \quad (11)$$

As K has been assigned a sliding value, in those cases where QE_1 differs from QE_2 the factor K will alter in value progressively from that appropriate to QE_1 to that appropriate to QE_2; and the mean value of K between these limits has to be used in the solution of the equation (11). This mean value of K will therefore depend to some extent on the rainfall R.

The curve of K (curve B) shown on *Fig. 5* gives the value of K for various levels where $QE_1 = QE_2$, that is where K would have the same value at the beginning and end of the 24 hours under discussion, and the rainfall would just balance the consumption. The mean value of K for rises and falls in the consumption due to the varying conditions of rainfall have been calculated by the method of successive approximations given in Table 6.

Based on the foregoing, the values of QE_2 have been computed for various values of QE_1 and various amounts of rainfall, and are shown in abridged form in Table 3 so that the consumption can be read off by inspection from that of the previous day. For instance, if the prior consumption was 0·20 inch and the next day's rainfall was 0·40 inch, then the next consumption would be 0·222 inch, and so on. This process can then be continued for as long as may be desired. Although it entails some labour in preparation of the initial table, this method saves much time if extensive records of rainfall have to be converted into consumption,

Fig. 5



ASSUMED VALUES OF DIMINISHING FACTOR K FOR RIVER SHIN

TABLE 6

K	Consumption	Mean consumption
(1) Using value of K appropriate QE_1 to read off the curve B in *Fig. 5*.	$KQE_1 + R(1 - K)$ $= QE_2$(1st value)	QE_{mean}(1st value) $= \dfrac{QE_1 + QE_2\text{(1st value)}}{2}$
(2) Using value of K_{mean} appropriate to QE_{mean}(1st value) read off the curve B in *Fig. 5*.	$K_{mean}QE_1 + R(1 - K_{mean})$ $= QE_2$(2nd value)	QE_{mean}(2nd value) $= \dfrac{QE_1 + QE_2\text{(2nd value)}}{2}$

(3) Continue the above process. It is found that two or three such successive approximations are ample to determine the mean value of QE to two or three decimal places, and hence the appropriate mean value of K.

makes the actual working one of inspection and tabulation, and minimizes arithmetical errors.

In *Fig. 5* are also shown the curves of the mean value of K appropriate to the recession in consumption at time of no rain (curve A), and the mean value of K if the rain is assumed to be 1·00 inch (curve C). The value of K appropriate to any condition of rainfall can of course be found from Table 3.

If K is considered as the mean value of the numerous run-off coefficients which would be applicable to the various parcels of land and types of

physical features which go to make up the whole catchment, it might be presumed to have different values at different times such as :—

(1) Frozen ground overlain with rapidly thawing snow.
(2) Ground covered with large seasonal variations in the depth of grasses and other plants.
(3) Ground either heavily planted or denuded of trees; heavy burning of the heather, improvements in land drainage, etc.
(4) Alterations in the shape of the weir at the outlet to Loch Shin or deliberate regulation of the river, etc.

As conditions in the River Shin area have been fairly static for many years, the assumption that a general run-off coefficient for the whole area can be applied to the rainfall over a long period appears to be reasonable in this instance.

Hydrograph of Daily Consumption and Flow in the River Shin

The result of the application of the foregoing principles to deduce the consumption from the rainfall, for the years 1947 to 1949 is shown plotted in *Figs 6*, *7*, and *8*. The corresponding hydrograph of the mean daily flow of the River Shin as actually observed (that is as deduced from the mean daily gauge height readings) is also shown. It will be seen that it is possible to apply a formula to the rainfall (irrespective of the time of year, etc.) which follows the variations in the observed flow with considerable regularity.

In order to compare the consumption derived from the rainfall with the daily flow, the latter has been reduced to flow in inches per day over the catchment area. In this respect an average flow at the rate of 5,254 cubic feet per second (or 2,830 million gallons per day) is equal in volume to 1 inch of rain running off per day over the catchment area.

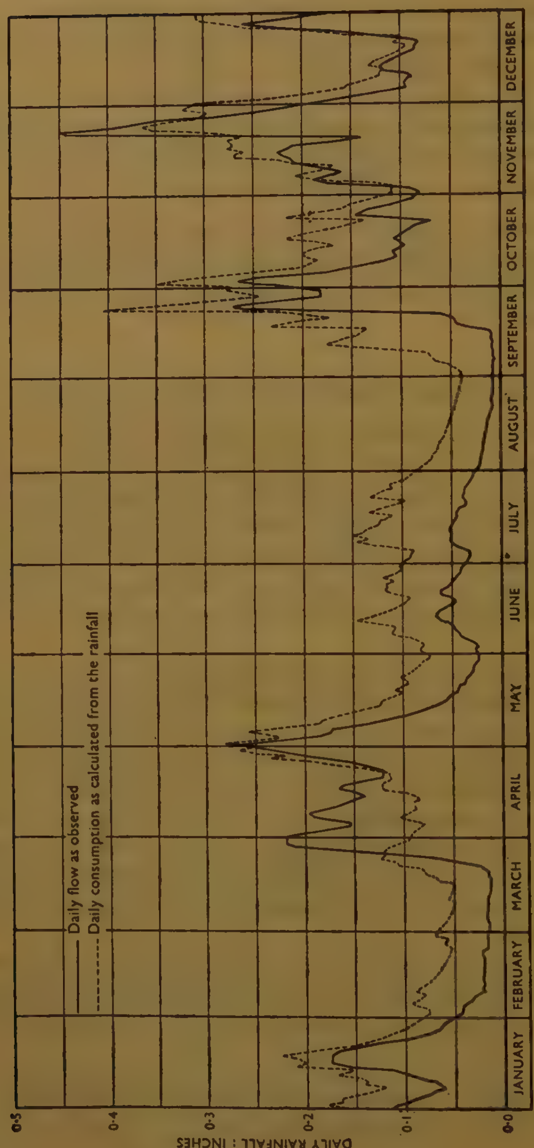
Occurrences of peaks and changes in river flow show that they occur one day after the corresponding rain. This is, of course, a condition necessary to satisfy the requirements of the formula developed from *Fig. 3*, where it is seen that the consumption due to any rainfall R on day n, occurs on day $n + 1$.

In carrying out the computations, the offsetting of the effect of the rainfall by one day has been done by listing the given quantities of rain as having fallen one day later than shown on the rainfall record.

Vertical Interval between the Curves of Consumption and Observed Flow

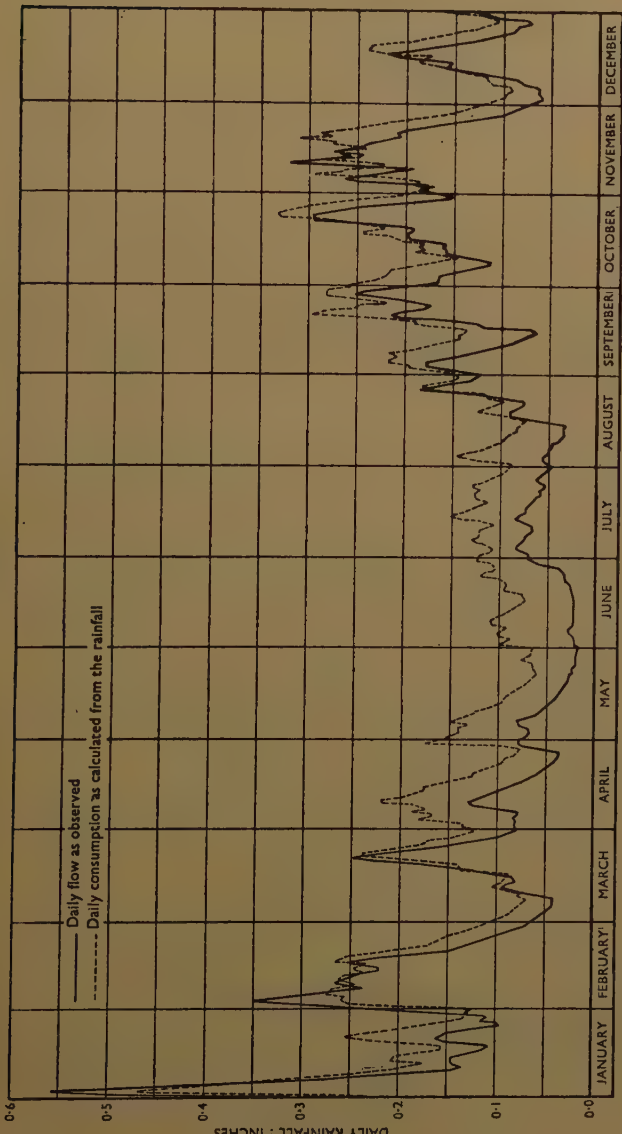
As the run-off over a long period must necessarily approximate to the total rainfall over the same period, less the avaporation losses, it follows that the curve of consumption of daily rainfall should lie above the

Fig. 6



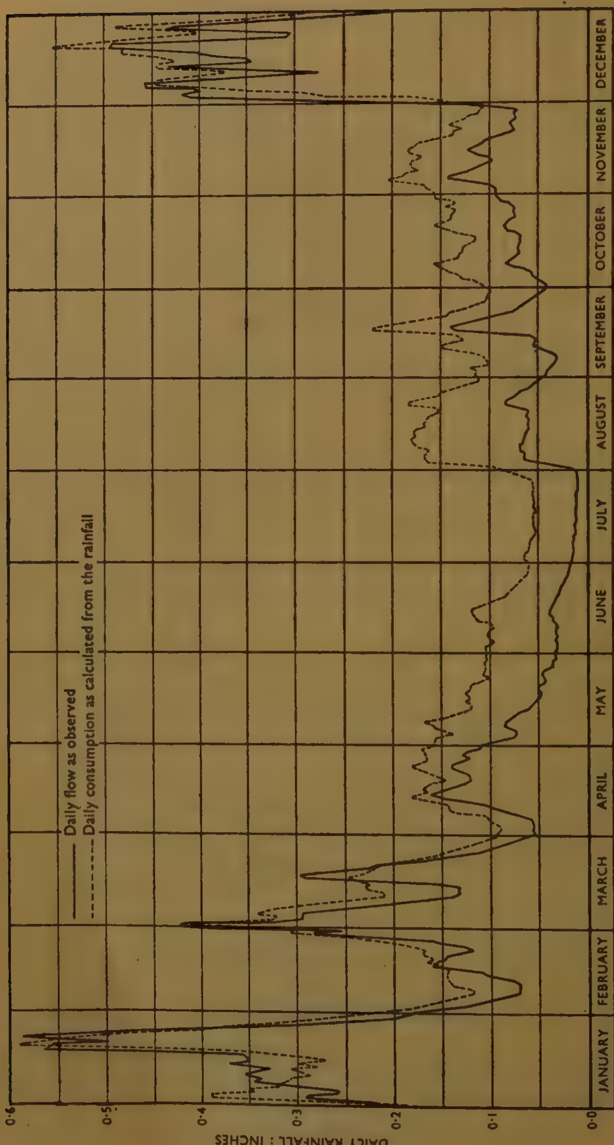
HYDROGRAPH OF THE DAILY CONSUMPTION ON THE RIVER SHIN AREA AS CALCULATED FROM THE RAINFALL OF THE YEAR 1947

Fig. 7



HYDROGRAPH OF THE DAILY CONSUMPTION ON THE RIVER SHIN AREA AS CALCULATED FROM THE RAINFALL OF THE YEAR 1948

Fig. 8



HYDROGRAPH OF THE DAILY CONSUMPTION ON THE RIVER SHIN AREA AS CALCULATED FROM THE RAINFALL OF THE YEAR 1949

hydrograph of the daily flow, and the vertical interval between the curves shown in *Figs 6*, *7* and *8* should be some measure of the actual daily loss by evaporation.

There are, however, a number of factors which go to modify the determination of the actual loss by the above means, such as :—

(1) The proper assessment of the rainfall.
(2) The correct measurement of the river flow.
(3) The proper assessment of the factor K.
(4) The effect of snow on the consumption.
(5) The effect of springs and seepages either bringing water in from, or leading water away into, other catchment areas.

Cases (1) and (2) have already been considered on pp. 3–7.

In the case of (3) alteration in the value assigned to K either accentuates the apparent rises and falls in the consumption or damps them down. It has thus a considerable effect on the day to day value of the consumption as calculated, both at intermediate stages and peak floods, which then has to be allowed for in assessing the appropriate evaporation; but it does not affect the average height of the consumption curve or the total area under it, if taken over a long period.

In the case of (4) the effect of snow, which is recorded in the rain gauges as its equivalent depth of water on the day on which it falls, can be seen from the hydrograph for 1947, *Fig. 6*. In 1947, snow fell in January and February, followed by a severe frost when it might be expected that the evaporation was very little. Yet the consumption curve is seen to lie well above the curve of actual flow. In March there was a rapid thaw without much actual rain and the flow is shown as much greater than the corresponding consumption, clearly showing the effect of the melting snow. The equivalent snow fall as recorded in the rain gauges, the amount of snow deemed to be banked during the frosty period, and the estimated run-off due to the melting snow for the early part of 1947 are as follows :—

Snow stored, as indicated by the observed flow curves; 1st Feb. to 17th March : 2·64 inches

Additional run-off due to melting snow, as indicated by the observed flow curves; 18th March to 20th March : 2·34 inches

Melting snow has, therefore, an effect which cannot be allowed for in the system under discussion as it stands. All that can be inferred is some measure of the total snow fall, if the actual flow is known. The method may possibly have some use in this connexion, as the total actual snow cover is difficult to measure by direct means.

In Sutherlandshire there are not usually any hard and fast seasons of snow and thaw, and this was the case in 1948 and 1949, and it is felt that the subject, as far as the River Shin is concerned, is too complex to handle at this stage.

In regard to (5), in the Shin district there is little likelihood of springs without the areas having any effect, but in some districts seepage either into or out of the catchment might be a major factor affecting the calculation of the losses, and might produce either an excessive, or even an apparently negative evaporation, the characteristics of which might require separate treatment.

Annual and Monthly Rainfall; Consumption Compared with Observed Flow for 1947 to 1949; Evaporation

The annual and monthly rainfall, the annual and monthly consumption derived from the daily rainfall, and the observed total annual and monthly flow of the River Shin, are given in Table 5. The annual and monthly loss by evaporation for the three years 1947 to 1949, is also shown, assessed as follows:—

(1) By direct subtraction of the observed flow from the monthly rainfall.

(2) By subtraction of the observed flow from the monthly consumption.

The annual and monthly consumption are derived from summation of the daily figures calculated by the tabulation method referred to on pp. 10–17. The rainfall figures are derived from the daily rainfall records as described on pp. 3–6. The consumption and flow figures in Table 5 are all given in inches of rain per day over the catchment area, for easy comparison with the rainfall.

As will be seen the evaporation losses derived by process (2) are more regular both monthly and annually than those derived from process (1), and frequent negative values for the evaporation are avoided. It will be seen that the evaporation is larger in summer than in winter, and it may be assumed that the evaporation derived by process (2) shows some fair approximation to the actual monthly losses which cannot be derived with such uniformity by any other means.

Average Values of the Daily Evaporation Losses

The vertical distances between the hydrographs of consumption and daily flow for each day of the three years 1947, 1948, and 1949, have been analysed on the assumption that this vertical distance is representative of the actual total daily evaporation loss; special conditions such as occurred in the spring of 1947 being omitted.

The loss by evaporation, in addition to being related to atmospheric conditions, must also be a function of the wetted area and therefore of the storage; and as storage and consumption are directly proportional, it is convenient to assess the evaporation as a function of the consumption.

In *Figs 9* (*a*), (*b*), and (*c*) the daily evaporation losses, plotted against the corresponding consumptions, are shown for various representative months of 1947, 1948, and 1949, the remaining months showing similar characteristics. There is seen to be, especially in the summer months, a very definite relationship between consumption and the daily evaporation loss irrespective of what may be presumed to be daily variation in climate. In winter, and at time of high river flow, the scatter in the points as plotted becomes very large, but the general trend for the consumption to be roughly related to the evaporation is apparent.

In general also it is seen that the evaporation in the summer is decidedly higher than in winter, and that there are times, especially in summer, when the evaporation tends to rise with consumption continuously, and that in winter, spring, and autumn, there is a tendency for the evaporation first to rise after a period of no rain and then to fall, presumably to zero, after prolonged rainfall.

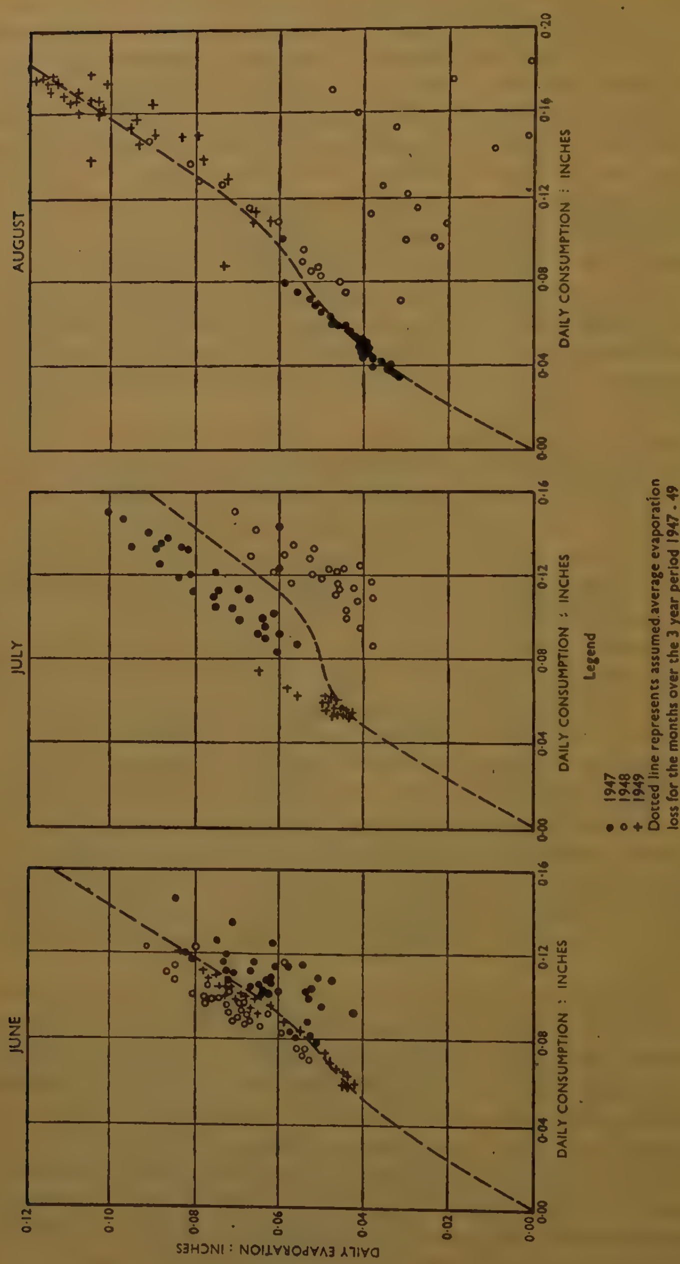
In some instances there is seen a negative loss ; this must either be caused by melting snow or by increase in the rainfall rate over the whole area at time of heavy falls, presumably on the higher ground which is not recorded in the rain gauges. Negative evaporation might arise as a calculation feature if the factor K has not been selected correctly. As it contains a logical paradox (if all the factors have been properly assessed) negative evaporation has been discounted for the time being.

As there has been in general a rough tendency for the evaporation in the corresponding months of 1947, 1948, and 1949 to follow the same trend, it was thought that average evaporation figures, based on the appropriate level of the consumption, could be prepared for the various seasons ; and could be assumed to be applicable in general for all years. An attempt was therefore made to assess average daily evaporation curves for the area, based on the mean of the 3 years under review, but owing to the scatter in the points the problem has been found to be remarkably difficult, as only very rough approximations to the true daily loss can be found by this means for certain. The average daily evaporation for a few months is shown in *Figs 9*, and for all the months of the year in *Fig. 10*.

The method employed to arrive at average curves for the daily evaporation as a function of the consumption was as follows :—the difference between the consumption and the observed flow for each calendar month of the year under review was plotted against the consumption on separate sheets of squared paper, and a curve to fit the points as nearly as possible was drawn in by hand. These curves are necessarily only approximations, as for the sake of simplicity and convenience the humidity, wind, temperature, cloud cover, and seasonal differences in the demands of plants have been ignored as separate factors, except in so far as the seasons are divided roughly by the adoption of suitable curves for the calendar months.

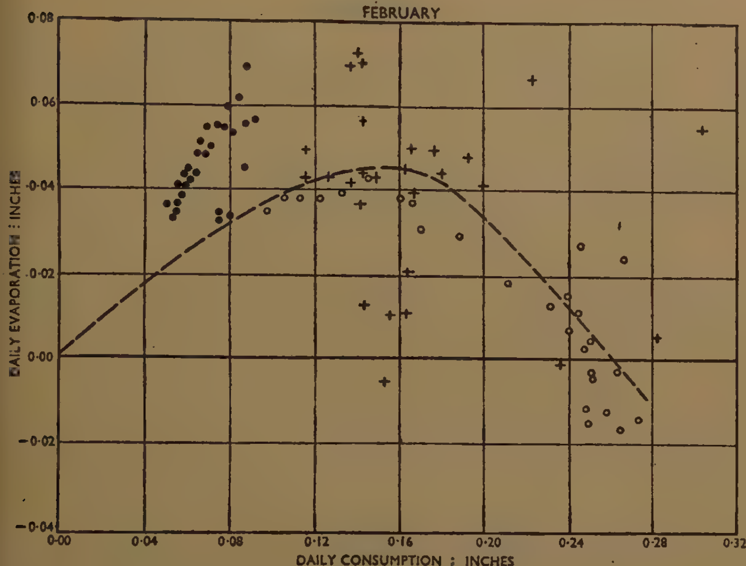
In spite of this the curves of assumed average evaporation show

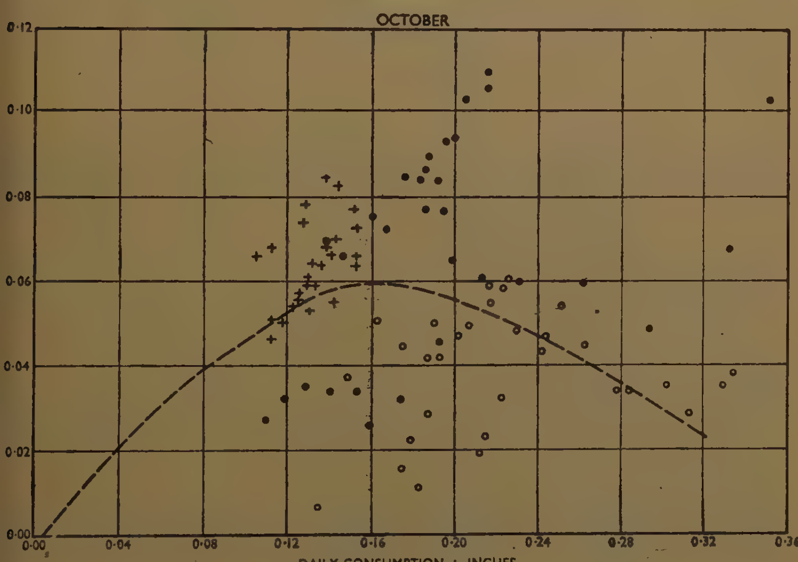
Figs 9 (a)



DAILY EVAPORATION FROM THE RIVER SHIN AREA AS A FUNCTION OF THE DAILY CONSUMPTION OF RAINFALL FOR VARIOUS MONTHS OF THE YEARS 1947, 1948, 1949. (See also *Figs 9 (b)* and *(c)*)

Figs 9 (b)



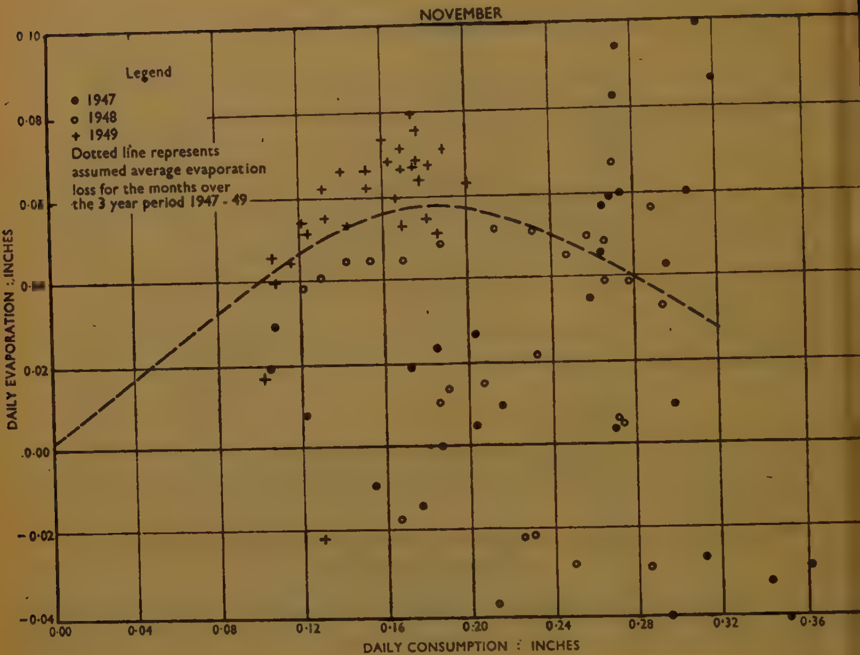


Legend

- ● 1947
- ○ 1948
- + 1949

Dotted line represents assumed average evaporation loss for the months over the 3 year period 1947 - 49

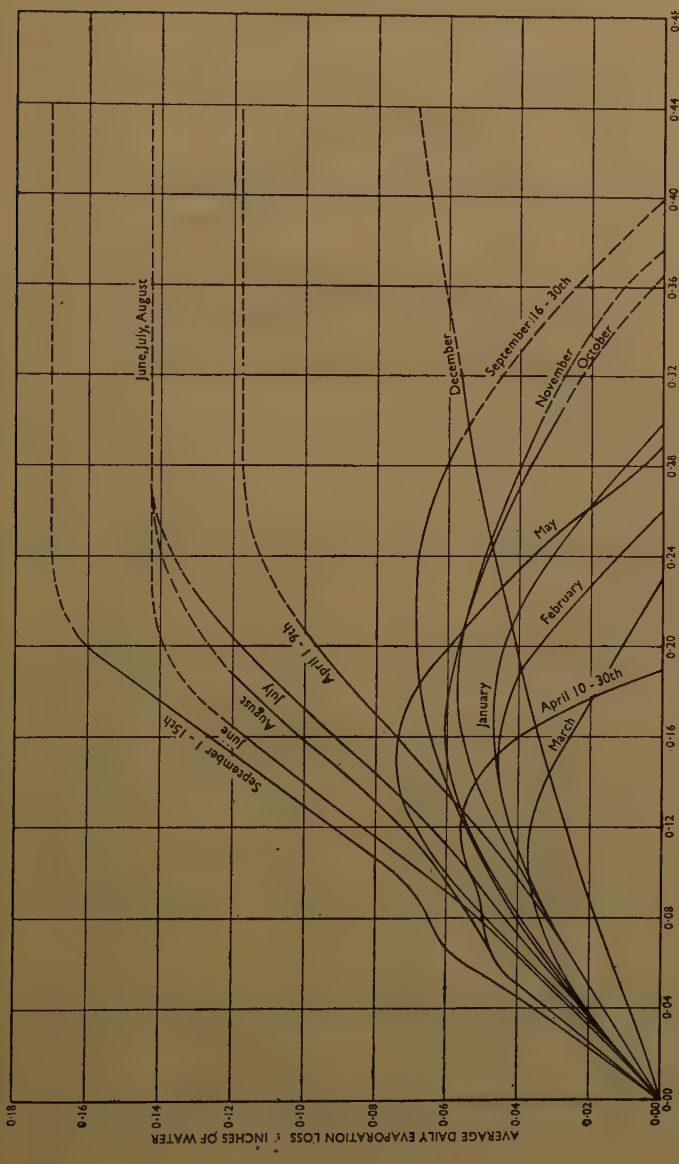
Figs 9 (c)



general agreement with what is already known about evaporation [6]—that it is low when the rainfall is low and increases with rainfall. It has been assumed, however, that especially in the winter with prolonged heavy rain, and great humidity in the summer, the evaporation and transpiration decrease, probably to zero.

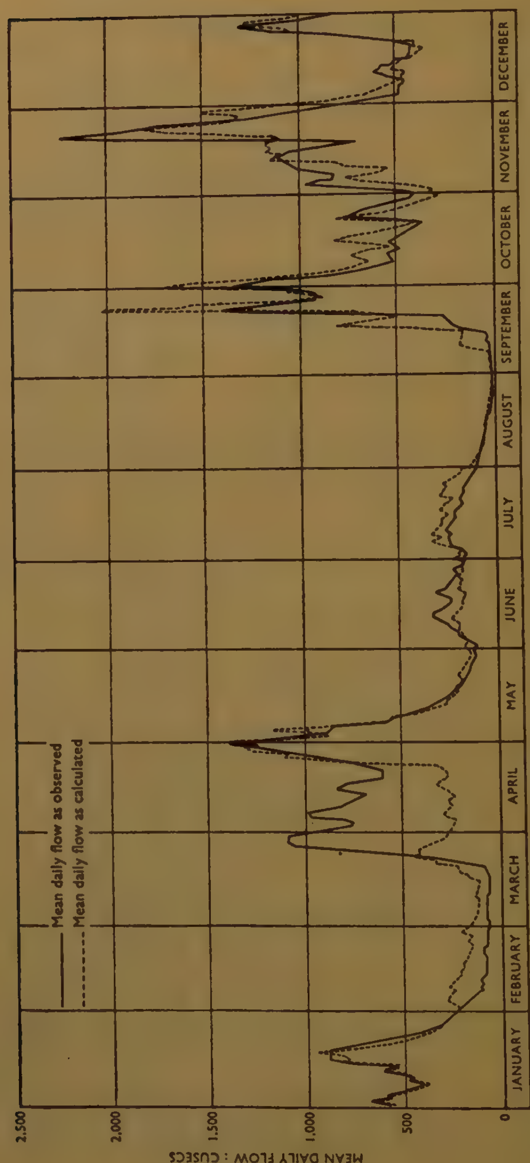
It is thought that if the above method of rating daily loss by evaporation could be extended to include various categories of climate or weather periods, based on the available records of daily temperature, humidity, wind, etc., a much more reliable concordance between observed and calculated flows would be possible. Standard curves of evaporation against consumption could then be varied both for time of year and for weather conditions. The daily rainfall records could then be divided, not so much into calendar months, as into periods dependent on weather experienced, and treatment by the method of tables prepared beforehand could be applied to the calculated consumption to obtain the daily flow, in the manner already indicated. The only complexity being the larger number of tables required and the judgment necessary in classification of the climatic periods.

Fig. 10



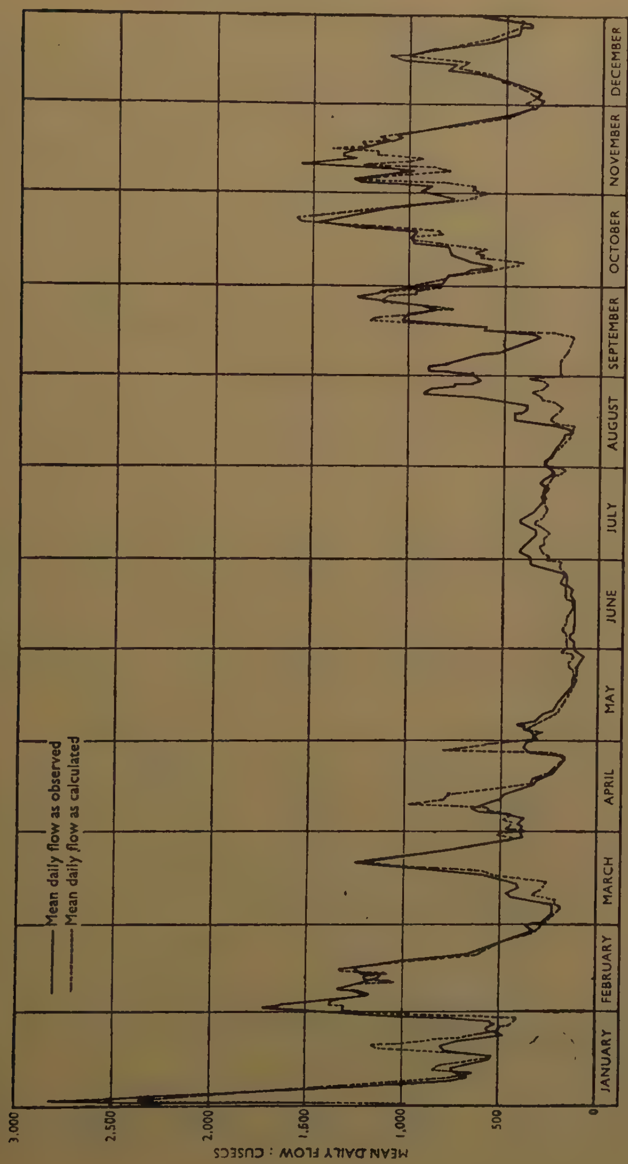
River Shin: Average Daily Loss by Evaporation for each Month of the Year, Based on the Average Daily Evaporation for the Years 1947 to 1949, Plotted as a Function of the Daily Consumption of the Rainfall

Fig. 11



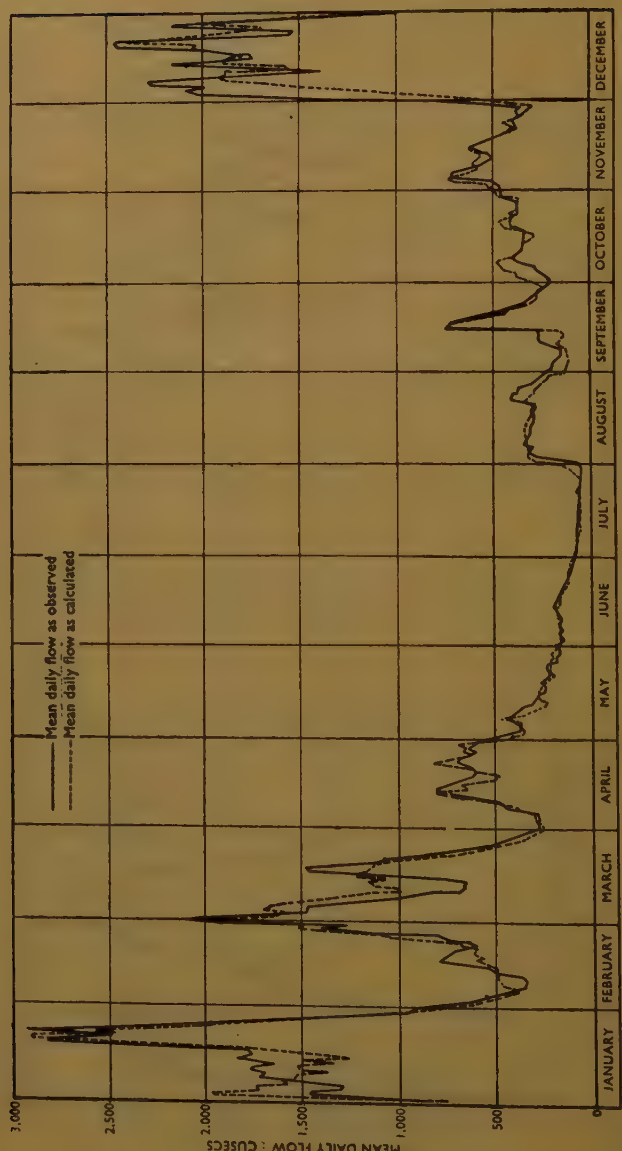
MEAN DAILY FLOW OF THE RIVER SHIN FOR THE YEAR 1947, AS OBSERVED AND AS CALCULATED FROM THE RAINFALL

Fig. 12



Mean Daily Flow of the River Shin for the Year 1948, as Observed and as Calculated from the Rainfall

Fig. 13



MEAN DAILY FLOW OF THE RIVER SHIN FOR THE YEAR 1949, AS OBSERVED AND AS CALCULATED FROM THE RAINFALL

Comparison of Calculated Flow with Gauge Readings

If any period, for which the rainfall is known, is taken and the consumption calculated, the flow can be deduced approximately by subtracting therefrom the average daily evaporation, derived as explained on pp. 22–26. The numerical value of the factors required to determine the daily flow from the rainfall, having been tentatively established as averages for the River Shin for the years 1947 to 1949, a check was made of these years to see how river daily flow, as calculated by means of these averages for the above years, compared with the observed figures. *Figs 11, 12,* and *13* show the daily river flows for the above years both observed and calculated. It is seen that on the whole there is fair agreement between the two; the concordance for the year 1948 being especially good.

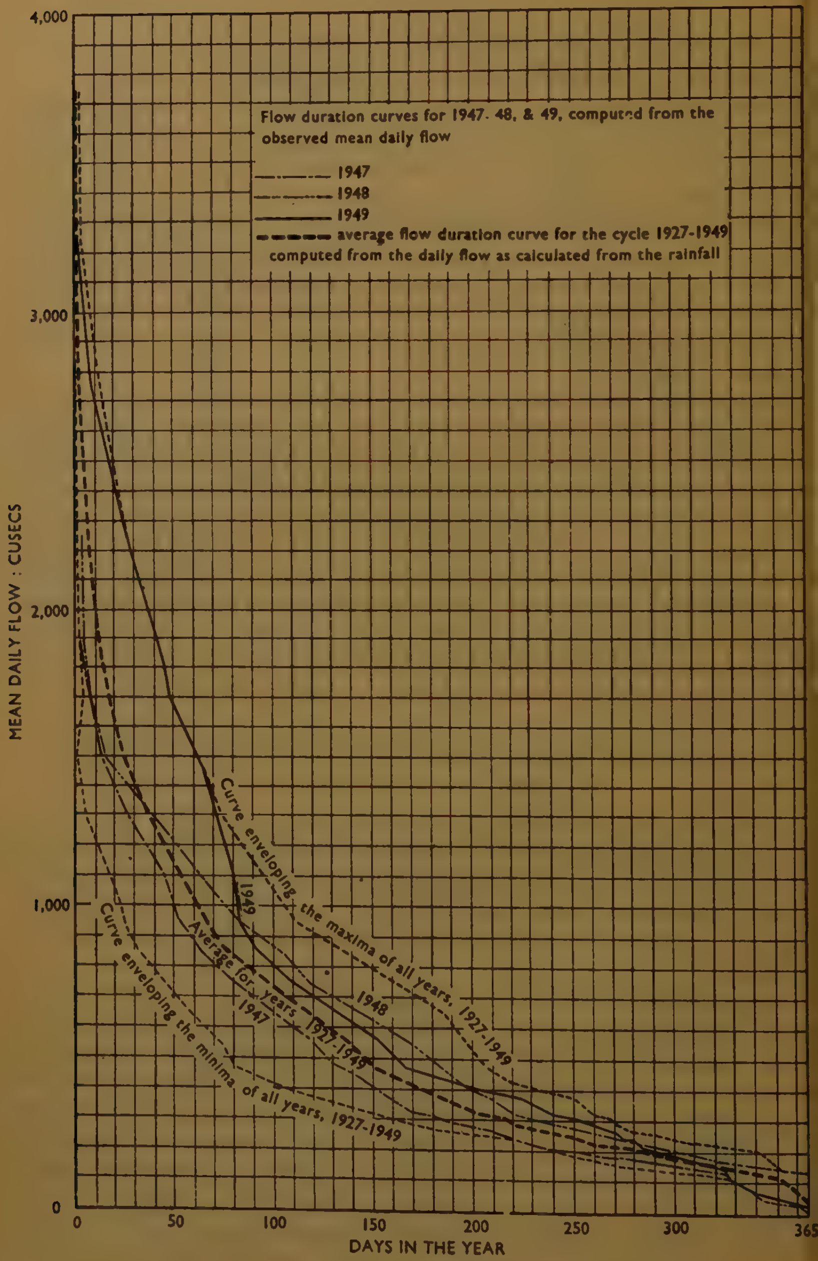
Except for snow, which greatly affected the result in early 1947, it is thought that the factors for the Shin have been determined closely enough to permit river flows, and consequently the mean daily level, to be predicted from the rainfall alone, without undue error, on the majority of days. In this respect, apart from the variables referred to on p. 23, there is always a source of error in the observed flow from Loch Shin due to the effect of wind, the reach being 18 miles. The effect on the daily flow due to wind, is very difficult to assess, as distinct from changes in level due to variations in inflow. The inflow cannot be measured directly and can only be inferred from the outflow and changes in loch levels and no conclusion has, so far, been reached on the actual effect of wind on the outflow.

Estimation of the Daily Flow in the River Shin for the Years 1927 to 1946

The estimated daily flow for the years 1927 to 1946 has been worked out by calculation based on the method outlined in this Paper, using the value for the factor K and the averages for the seasonal evaporation as determined for the years 1947 to 1949. The effect of banked snow on the daily values of the flow has been ignored in these calculations for the time being.

Approximate figures for the maximum–minimum and average daily flow, and the flow in the 3 driest months, etc., can be obtained from these calculated flows. These figures cannot be deduced with any degree of reliability from the rainfall alone, unless some method such as that explained in the foregoing pages is adopted. They can only be estimated very approximately from the general formulae at present in use, and, of course, no day to day picture of the flow, however approximate, can be obtained for past times by any of these latter means. The more certain method of direct measurement of flow will require many years of observation, a condition which, as in the majority of cases of this nature, there is no time to fulfil.

Fig. 14



FLOW-DURATION CURVES FOR RIVER SHIN, 1927–1949

Flow duration curves (number of days in the year on which the flow reached stated values) for the years 1927 to 1946 have been derived from the calculated flows and were plotted together with the observed flow duration curves for 1947 to 1949. These are shown in *Fig. 14.* From these curves it will be seen that the daily flows, as calculated from rainfall by the foregoing method for the years 1927 to 1946, are in agreement with what might be expected from the flow duration curves plotted from the measured flows in 1947 to 1949, bearing in mind the variation in the rainfall from year to year as employed in the calculations.

Recession Curve of Flow and Run-off Factor for a Reservoir with a Fixed Weir at the Outlet

As previously stated the recession curve of daily flow from an area is considered as logarithmic, with a progressive increase in the factor K. The mean value of K, applicable between any two successive levels of the stored volume, being taken as the proportion

$$K = \frac{\text{volume remaining in storage at the end of the day}}{\text{volume in storage at the beginning of the day}}$$

This, as was shown on pp. 7–10, can also be written

$$K = \frac{\text{total flow in 24 hours}}{\text{total flow in the preceding 24 hours}}.$$

The curve for K has been deduced empirically for the Loch Shin catchment, but it can be shown that a similar curve exists for the recession of flow from any reservoir (if not replenished by inflow) with a fixed weir at its outlet.

For this calculation Loch Shin can be assumed to be a reservoir of 10 square miles area with vertical sides, and with no inflow from the surrounding land.

The weir at the outlet to the Loch has not the characteristic of a free overfall weir as it is of rough shape and is partially drowned. It is found, however, that the following formula fits the discharge over the weir as inferred from the discharge measurements taken by current meter for all states of the river, both high and low :—

$$Q = 120(d + 0{\cdot}50)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where Q denotes discharge in cusecs
and d ,, depth of water over the cill in feet.

Assuming $D = (d + 0{\cdot}50)$

and D_1, D_2, etc. denote the water levels on successive days, the total flow, F_1, F_2, etc., in each successive day is given by the equation

$$F_1 = (D_1 - D_2) \times 10 \times 5{,}280^2 \text{ cubic feet} \quad . \quad . \quad . \quad (14)$$

A similar relationship for the flow on successive days holds good and the factor K is then given by the ratio $\frac{F_2}{F_1}$.

The formula for the emptying of the reservoir of area 10 square miles in 24 hours over a fixed weir whose discharge is given by the formula $Q = 120D^2$ is as follows :—

$$\frac{1}{D_1} - \frac{1}{D_2} = -\frac{120 \times 24 \times 3{,}600}{10 \times 5{,}280^2} = -0{\cdot}037 \quad . \quad (15)$$

hence

$$D_2 = \frac{D_1}{1 + 0{\cdot}037D_1} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The total flow, F, in 24 hours thus becomes

$$\left(D_1 - \frac{D_1}{1 + 0{\cdot}037D}\right) \times 5{,}280^2 \times 10 \quad . \quad . \quad (17)$$

From the above the ratio of $\frac{F_1}{F_2}$, etc., can be worked out for various levels of flow and the result is shown in the curve D plotted on *Fig. 5*.

The curve B for K, shown in *Fig. 5*, is of course the curve of the mean value of K for conditions of receding discharge over the weir when the run-off factor is continually changing, as the head over the weir decreases.

For conditions where there is inflow into the reservoir which would just balance the outflow the factor K represents the following relationship :—

where

V denotes volume in the reservoir.

F ,, outflow over the weir at constant head.

Then the run-off coefficient is given by the formula

$$K = \frac{V - F}{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

The curve connecting points for K calculated for various levels over the weir for the above condition, is given as curve E in *Fig. 5*, and is, of course, comparable to the curve for K (curve B, *Fig. 5*) for the whole catchment area for those conditions when the rainfall equals the consumption. It is thus seen that there are in effect several curves for K dependent on the conditions; the curve for conditions of constant head being considered the standard curve for the run-off factor K, and the other curves being those for the mean value of K, applicable to those conditions when the factor is changing in value, due either to no rainfall, rainfall too little in amount to prevent a continued fall in flow, or rainfall heavy enough to cause the flow to rise.

The difference between the curves B and E (shown in *Fig. 5*) of the

factor K for the above ideal case (inflow equalling the outflow), and that found empirically to suit the actual conditions, is due to the effect of the inflow into the loch from the surrounding catchment area. This inflow, of course, may be presumed to undergo a progressive depletion on occasions of no rain, and an increase due to rain, in a similar manner to that set out in the Paper for the whole area. The run-off factor K for the inflow will be much smaller than that for the area including Loch Shin, due to the small storage power of the bare hill sides. The factor K as applicable to the whole area, may be taken as an average of all the various run-off factors which may be operating.

General Application of the Method to Flood Hydrographs

The method, as will be seen from the development of the converging series set out on pp. 12–13, bears some resemblance to the unit hydrograph [7, 8] in which successive empirical percentages such as 15, 40, 35, 10, etc., are applied to the rainfall in order to determine the flow, but was in this instance derived independently. The adoption of a sliding scale for the factor K, dependent on the storage level, and a simple tabulation method of computing the day to day value of the flow carries the idea one step further. The idea of a fixed base flow, as used in the unit hydrograph method, can be discarded. It is merely that in the case of the more uniform flow from gravel beds and other subsoil waters, peat bogs, etc., the factor K is near unity, making the flow very nearly the same every succeeding day, and it is at low flows that the concept of base flows comes into prominence.

The flow due to melting snow cannot be inferred from the consumption curve. The usefulness of hydrographs developed by the above method is therefore severely limited if applied to winter months, unless some means of determining the banking of snow and its rate of dissolution on thawing can be reached. At times of rapid thaw there may be, however, the additional complication of an unpredictable temporary improvement in the run-off factor, K. Similarly a further study of the climate as related to evaporation, and if possible the division of the annual periods into various rough categories, will go far to improve the average evaporation curves shown in *Fig. 10*.

By use of the formula developed on pp. 7–13, it is possible to show some well-known features of river flow which can be very clearly explained thereby.

For instance, it shows that in the limit, the maximum flow will be equal to the rainfall (neglecting evaporation).

Consider a daily rate of rain, R, to fall continuously ; then the consumption on each day will be given in Table 7.

As K is fractional, the term $(1 - K^n)$ will in the limit approach unity, and thus rainfall and consumption will eventually be equal.

TABLE 7

Day	Rainfall	Consumption each day
1	R	$R(1-K)$
2	R	$KR(1-K)+R(1-K)=R(1-K^2)$
n	R	$KR(1-K^{n-1})+R(1-K)=R(1-K^n)$

Similarly, for any given flow to be maintained, the daily rainfall must equal the daily consumption. Consider two consecutive days when the rainfall, R, on the second day just balances the consumption :

by formula (11) the consumption is given as

$$QE_2 = KQE_1 + R\,(1-K)$$

If $QE_2 = QE_1$; the above can be written

$$QE_1 = KQE_1 + R(1-K)$$

hence

$$QE_1 - KQE_1 = R(1-K)$$

and

$$QE_1(1-K) = R(1-K)$$

then

$$QE_1 = R.$$

The rainfall on any subsequent day must be equal to the consumption on the prior day if the flow is to be maintained. It is also seen that this requirement is independent of the value of K, and thus holds good for all levels of consumption and rainfall.

Also explained is the lag in the time of peak flow behind the time of maximum rainfall intensity, for if the rain begins to fall, and then after a period the intensity is reduced, the flood will continue to rise until the rate of flow becomes equal to the intensity of the rainfall, after which, if the rainfall intensity continues to decrease the flood will also commence to subside.

To illustrate this point in *Fig. 15* are shown curves of flow developed for hypothetical storms of,

(1) 9 inches of total rainfall in 24 days rising to the rate of 1 inch per day in the middle of the storm.

(2) 9 inches of total rainfall spread evenly over 24 days.

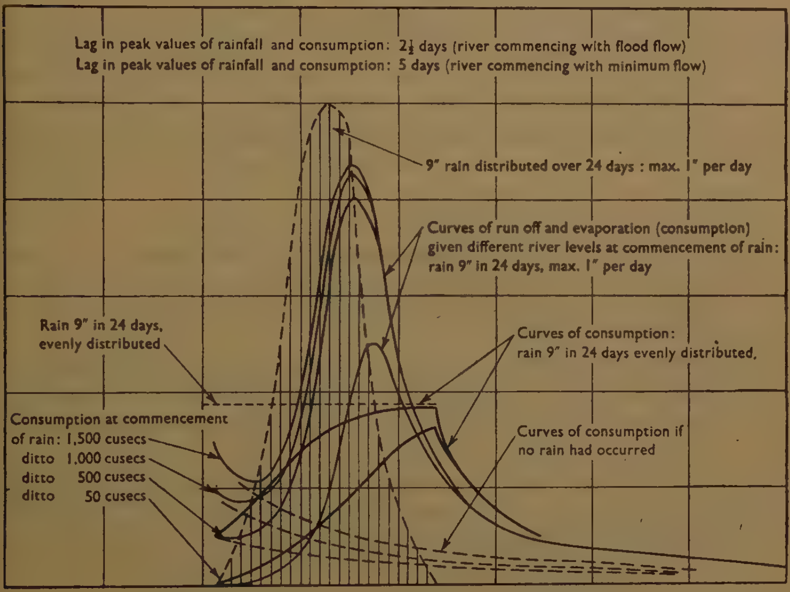
The value of the factor K adopted for the River Shin has been used.

It also explains why the resultant flow from any given rain is greatly affected by the prior state of the catchment, and makes it possible to determine the resultant flow quantitatively for any given prior set of conditions.

Various curves are plotted to show the effect of prior rainfall or drought, and therefore saturation or otherwise of the ground. It can be seen that when the flow is initially small, the rise in flow due to subsequent rain is

much less than the rise in flow due to rain when the initial flow is high. It can, moreover, be seen that there is a limiting factor to the process,

Fig. 15



Correlation of Daily Rainfall with Run-off

and that when the flow is very high a continued high rainfall is necessary to maintain it at a steady level.

Conclusion

The formula developed on pp. 7–13 has been adopted for its simplicity in tabulation of the calculations by a step by step process.

It can be seen that once factors K and E have been established by reference to the observed flows, the calculated flows are derived solely from the rainfall without any further reference to the observed conditions. Further, it is seen that the process of computation is continuous and absolutely free from any cumulative errors.

Discrepancies in the flow as obtained by calculation, as compared with the actual observed flow, are probably due to unpredictable changes in the flow due to changes in the level of Loch Shin caused by wind. There is also the difficulty in assessing the value of K, and the average daily evaporation losses, which may have seasonal or accidental variations which cannot be properly allowed for by assuming that an average figure for the evaporation is applicable in any particular day or season. The daily

rainfall for such a large area, and the variation in rainfall intensity for the whole area within the 24-hour period, are also factors of which accurate assessment is probably impossible.

It will also probably be impossible to measure the actual variation in hourly rainfall intensities as they bear on river flow, and it seems clear that the establishment of the relationship of rainfall to run-off for periods of less than a day cannot be attempted for large areas. This means that the method outlined in these pages cannot be used to calculate peak flows from rainfall if the duration of the peak is under 24 hours. All that can be shown is the relation of the total daily amount of rain to the total daily flow.

In the case of the formula developed on pp. 7–13, the rain is assumed to fall all at once, at the beginning of the day, no allowance being made for the gradual rise in flow due to the rain being distributed over the whole day. This is an approximation which does not vitally alter the general principle.

Unfortunately, if for instance the hourly distribution of rainfall was known, then the large increase in the number of calculations necessary to study the hydrograph would, in practice, make the matter very cumbersome.

The method described in this Paper leads, however, to a surprisingly consistent determination of the relationship of rainfall to run-off, in spite of the uncertainties inseparable from any investigation where so many factors, for the most part unmeasurable, have to be taken into account.

Acknowledgements

The Author is indebted to the late Sir Edward McColl, who, when Deputy Chairman of the North of Scotland Hydro-Electric Board, gave permission to use the data in possession of the Board and to publish this Paper.

The Author also wishes to thank the Scientific Computing Service and Messrs G. R. Hoffman, B.Sc., A.M.I.C.E., and D. M. Cameron, for their assistance in the laborious work of reducing the daily rainfall into daily flow over the period of 23 years covered by the Paper.

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The Paper is accompanied by eleven sheets of drawings and seven Tables from which the Figures and Tables in the text have been prepared.

Discussion

The Author introduced the Paper with the aid of lantern slides.

Mr Gerald Lacey said that if the method used by the Author in the case of Loch Shin could be applied to other similar catchments it would prove to be a great advance. The Author had been successful in achieving his main object, the reconstruction of the River Shin hydrographs from past rainfall records, and those were valuable when applied to the solution of the type of problem which hydro-electric development presented. The Paper had made it very clear that Loch Shin, which was effectively a measuring tank, had played a great part in assisting the Author to reconstruct the hydrographs.

The Author had referred to a recession curve applicable to the catchment as a whole; but there was no doubt that the Loch, acting as a measuring tank, had a recession curve of its own, and it was that curve that had had a dominating influence on the entire analysis which the Author had carried out. The recession curve for the Loch itself could not be represented by a logarithmic equation. The Author, in his Paper, had frequently referred to logarithmic recession, and Mr Lacey thought that the Author might have saved himself some labour had he followed up that idea, and adopted some form of logarithmic approximation.

In illustration of his remarks Mr Lacey drew attention to *Figs 3* and *4* of the Paper. It would be seen that after an interval of 1 day an initial storage of R was reduced to a value of KR in which K was termed by the Author a diminishing factor. It was simpler, however, to deal in terms of the rate of change of R, the storage, with respect to T, the time; and if that were done and the rate assumed to be constant a simple logarithmic equation would be obtained. Thus:

$$-Q = dR/dT = -\alpha R,$$

and on integration

$$R_T = R_0 e^{-\alpha T}$$

and

$$Q_T = \alpha R_0 e^{-\alpha T}$$

If those equations were employed, curves such as those in the Author's *Figs 3* and *4* were replaced by straight lines, if they were plotted on semi-logarithmic paper.

The Author had found that his diminishing factor K had to be varied according to a sliding scale, and that meant that the recession curve was not logarithmic. If the logarithms of Q were plotted vertically and the time T plotted horizontally, to a natural scale, the curve would be a straight line so long as the value of α was a constant, and it would be necessary to change the inclination of that line according to the range of discharge concerned. Thus when the Author was attempting to reproduce a known hydrograph for a given year he could have plotted it in that manner in the first instance, and "followed it up" by integrating from day to day when rain fell and applying an appropriate value of α for intervals without rain. That graphical method was much simpler than the computation of a converging series.

Reverting to the question of Loch Shin acting as a tank, Mr Lacey said that he had seen the catchment and there was little doubt that at times of heavy rainfall a very high percentage of the run-off found its way to the loch within 24 hours. Once that run-off reached the loch it was immediately linked up with the natural recession, or "time-of-emptying curve" of the loch itself, and that curve was different from, and by no means identical with, the type of curve that would be obtained if there were no loch and the water flowed off the catchment.

The simple logarithmic relationship which Mr Lacey had quoted applied rigidly to ground-water flow if that consisted entirely of seepage of which the flow was proportional to the head and to the assumed storage. It could however be employed, in the form of a series of chords, as a substitute for the true recession curve which, of course, had yet to be evaluated.

The Author's methods of analysis, with improvements in the technique, should advance the study of an important subject.

Mr W. M. McClean said that the real object of the survey was to record the run-off from 195 square miles by continuous records of stage water level. Mr McClean considered that that was the only basis for all run-off records; and the run-off record was the only volumetric measurement in the hydrologic cycle which had any considerable accuracy. Rainfall on an area could not be measured with such a degree of volumetric accuracy as actual run-off when that run-off was very carefully measured and continuous water-level records were maintained.

The measurement of water levels at 9 a.m. in the morning was considered to be sufficiently good on some rivers; but if those levels were taken at 9 a.m. on the River Dee, 1 inch too much run-off for the year would be recorded. The melting snow of the previous day arrived at 9 a.m. at the Cairnton intake to the water supply; and if the levels were taken at 9 a.m. that would be almost regularly the highest water level of the day.

In the spring months the daily rate of flow varied about 300 cusecs between maximum and minimum, a very big variation amounting to about 1 inch too much run-off in 3 months. He referred to Sir John Murray's plan of Loch Shin and its outlet, in the Journal of the Royal Geographical Society (1905). The range in level of Loch Shin had been taken as about 7 feet at the time of the survey. He did not know whether there was any later information on that matter. The water-level station at Lairg did not actually give the water level of Loch Shin; it was for the purpose of the stage-flow table of the out-flowing river. The outlet from Loch Shin itself was only 1 or 2 feet deep at low water on the 180-foot width of outlet.

With regard to the stage-flow table, the flow-gaugings were apparently not above 1,500 cusecs, or about 3/10 inch, per day. Was the logarithmic graph of the stage-flow curve a straight line at low and high levels, the rather extreme levels, and was the height scale for the stage-flow relationship at Lairg or at the gauging site?

The residual run-off or recession curve provided, from one dry period to another, a balancing figure between consumption and run-off; and in the Paper an adjustment for rainfall was made to the recession curve, which was really a measure of storage. During a dry period, the residual flow was entirely from temporary storages, in the lochs or in the river itself; after the rain had stopped, it was a measure of aggregate storage. The similar curves which he had himself obtained showed the varying changes in season and saturation. The Garry residual curve (the recession curve) was extraordinarily regular, owing to the big loch storage there as in the case of the Shin. The ground storage did not make so much difference. He had hesitated to try to connect daily consumption with estimated rainfall. He suggested that the term "net inflow" was a good one. The losses were deducted from the gross inflow, which was the rainfall, and net inflow was obtained.

On the matter of trying to correct the difference between apparent losses, which was an objective of the Paper, he had not himself gone further than graphical records of the available data, illustrated by a 6-months graphical record of the River Garry, which he placed on the table.

The objective of the Paper was a statistical correlation for the comparison of daily rainfall with daily consumption.

He considered that the important point was the loss of the annual cycle, which was 16·2 on the average for the 3 years. That was a high figure. When taking the rainfall on the Meteorological Office basis, he had generally found that the annual loss was considerably less; was there any alteration which had raised the rainfall on the Shin area?

Dr J. Glasspoole said that the distribution of the probable average annual rainfall, as given in *Fig. 1*, was not so good as that published in 1949 by the Ordnance Survey covering Great Britain on a scale of 10 miles to 1 inch, as prepared in the Meteorological Office. *Fig. 1* puzzled him, since the rainfall station at Lairg was shown within the area having less than 35

inches, whereas the average quoted on p. 3 of the Paper was 37 inches. There was not that ambiguity in the published map, which was available through the Ordnance Survey, and had been used for the rainfall values in the Paper where attributed to the Meteorological Office.

One great risk of relying on only two rainfall stations was that in the course of years a rain-gauge might become defective, the exposure might change, or the record cease, so that the homogeneity of the record was broken. When more stations were used, any such changes became readily apparent or could be rectified, and the continuity could be maintained.

It had been stated on p. 12 of the Paper that the effect of any specific day of rain would become negligible in about 2 months. Possibly that might be better expressed as "negligible as a first approximation." Owing to ground-storage properties, the effect on the run-off of a number of days with or without rainfall would still persist, even after a period much longer than 2 months.

On p. 22 it had been stated that loss by evaporation was a function of the wetted area, but it was the storage which depended on the wetted area as did naturally the run-off. The evaporation was much more independent, because there was always some moisture to be re-evaporated in that area and because it was controlled by the heat energy available through the meteorological factors of sunshine and temperature. The monthly distribution of evaporation given in Table 5 seemed to him unreal, and he thought there must be some storage factors still involved.

A slide was shown on which the mean monthly values of evaporation and rainfall as given in Table 5 were plotted. It was argued that the seasonal distribution of the values for evaporation was unreal, in that a much smoother curve, from very small values in January and December to the maximum in June and July, would be expected. The values in Table 5 for evaporation were clearly dependent upon changes in storage, the maximum value in September being due not to evaporation but to making good ground storage. Dr Glasspoole supported the argument by showing similar curves of the monthly means for the same period of the sunshine, in mean hours per day, and air temperature, in °F., for the nearest stations at Strathy and Achnashellach.

He hoped that the Author would apply his novel method to securing an even more detailed relationship, taking into account, separately if possible, the evaporation and storage.

On p. 23 reference had been made to what was already known about evaporation; but the reference had been to a book published in 1913. Much more had become known since then, and possibly the daily evaporation could be determined independently by the elaboration of the method introduced by Dr Penman, based on climatological records. Dr Glasspoole thought that what was required was an estimate, day by day, of the evaporation so that the complexities of the storage could be unravelled. Further progress might well be made along those lines. He confessed

that he had tried that in another area during the last few months but without any material success so far. He hoped that the Author would develop his novel method not only in the Shin area, but would test it out in other areas where the rainfall, the run-off, and the evaporation could be set out with greater precision, and balanced against the storage.

Mr N. J. Cochrane said that his first reaction on reading the Paper had been that the Author had chosen an exceptionally propitious site with a high rainfall, high run-off, a large regulating lake with a fixed outlet, and a catchment devoid of all but the most primitive types of vegetation and soil cover. However, as the Paper had progressed, a fairly well-trodden path had opened ahead, for the Author had introduced the well-known "variable constant," that bugbear of the art of hydrology.

The analysis had then pursued its way through the curves in *Fig. 9*, where the dry-season points conformed, though the wet-season ones did not, to *Fig. 10* which was more or less the climax. From that diagram it would appear that if the daily consumption was 0·20 inch, the evaporation on, say, the 14th September would be 2¼ times that on the 17th September! Nevertheless, in *Figs 11*, *12*, and *13* the Author had achieved a remarkable coincidence between observation and calculation.

Engineers were not always ruthless enough in their analyses of observations. Whereas statisticians were concerned only with noting patterns of behaviour, engineers were prolific in the production of symbolic formulae in which they found that they had to insert, as the Author had done, the "variable constants" to make theory and practice agree. It was all too easy for critics to discourage the triers, but it had been said with some truth of one of the Authors quoted in references at the end of the Paper, that if enough about the catchment were known to put values to his "variable constants," there would be no need to use his formulae. That was the real paradox of the Author's work, and indeed of all similar work, and he hoped the Author would give it a little thought. The Author's variables were really rainfall, season, and lake level, and he thought the Author could have achieved a lot with simple nomograms.

The Author had treated the isohyetal maps prepared by the Meteorological Office with considerable respect, as if they were almost the only rock in the bogs and quagmires of Glen Shin. But suppose they were not accepted without question; suppose they were dissected unsympathetically. It would be found that they were based on very few rainfall stations in an extremely rugged country. The isohyets were extended up and down dale by a mixture of theoretical and empirical methods. They were, in fact, the best that could be made of a very few known facts, and he would not expect their accuracy to be very high in the Highlands. He doubted whether that was the contention of the Office, but the isohyets were usually presented as thick black lines on a map and there was always the danger of taking them too literally.

In Glen Shin there appeared to be one rain gauge at the exit from the

loch and one other some 5 miles outside the catchment. He doubted if those would allow the drawing of accurate isohyets ranging between the extremes of 35 inches and 80 inches per annum.

That, however, was the basis of much of the Author's analysis and even that analysis only extended over 3 years. The Author was convinced that he could extend his analysis back into the past and into the future; but had he observed dry years and wet years, or mild years and hard years? Would he have to extend the range of his "variable constants"? Mr Cochrane thought that the Author would have to do that, and that the graphs of 1950 and 1951 showed that the deductions of 1947, 1948 and 1949 did not apply without modification.

Mr Cochrane said he hoped the Author would not take his criticism too much to heart, but all new hydrological theses had to pass through the fire and the Author could be sure that only the useless parts would be consumed.

Mr P. O. Wolf said that the Author's frank presentation and full discussion of his subject matter seemed, regrettably, to have called forth more criticism than constructive proposals. Although much of the criticism might be valid, he wished to emphasize that the Paper clearly contained the result of most painstaking and laborious computations such as, to his knowledge, had not been surpassed in the preparation of other projects of the size of the Shin scheme.

In the preparation of hydro-electric schemes, or any other water-engineering schemes, hydrographs were required to enable the water engineer to forecast data on which he could base an estimate of the output of his scheme and the design of his structures and other equipment. Among those data was the long-term average flow that could be obtained from the catchment; the reliable dry-weather flow (the minimum) which the water engineer could guarantee for a very large number of years; the flow which, on average, he could rely on obtaining for a given number of days in succession on a given storage and which would be more than the continuous guaranteed flow; and the peak flow for which he would have to design his structures to make them safe during their economic life.

There were two lines of attack on the problem. Both assumed that past trends in the meteorological and run-off behaviour of a catchment would continue unchanged in future.

One consisted of the analysis of a hydrograph and the associated curves and statistics. If run-off observations were too few in number, as in the case concerned, an attempt was made to deduce from the available meteorological data run-off values from which a long-period hydrograph could be constructed. That method was based on a full knowledge of the hydrological processes on the catchment, and that knowledge would usually be the result of exceedingly difficult studies.

There was another method, based on mathematical relations usually obtained empirically. It was less scientific than the first in that no proof

of the validity of the formula was available, but he thought it was a much speedier method in most cases than the first. As an example he mentioned a paper[1] by Dr Bamford, presented to the Institution about 20 years ago. Dr Bamford had obtained a useful formula connecting past rainfalls with past water levels in a river in Ceylon, without allowing in his derivation specifically for the geology, the topography, the botany, the various meteorological factors or even the run-off in the river. He appears to have successfully used his formula for flood forecasts.

While he fully supported the Author's ambition to derive formulae in which every step had a clearly understood physical meaning, Mr Wolf thought that the Author had been forced to adopt the second method: instead of introducing his formula (11) for a coefficient K in the calculation of what he termed daily "consumption," in order to obtain Table 3, he might have written his formula simply in the form of $QE_2 = f$ (QE_1, R_2) where f stood for "some function of." His study of evaporation did not lead to any clear conclusion except that there was a seasonal variation, and that might have been taken care of by extending the above formula to $QE_2 = f\ (QE_1, R_2\ Season)$. Admittedly f would be valid for one catchment only and would give less help in the study of other catchments than a thorough hydrological investigation, but the second method, at the present state of knowledge, seemed to yield engineering results much more speedily than the first.

Dr H. L. Penman said that the chief weakness of the Paper was in the unsatisfactory treatment of evaporation. As a result of work during the past few years, it was possible to state with confidence that the mean annual evaporation from the Shin catchment was close to 15 inches, that the year-to-year variation was small, that the precision of that figure was much greater than that of rainfall and was probably better than that for run-off, and that within a few tenths of an inch it was possible to write down the average monthly evaporation for the area. Those values were:—

Month	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year
E (inch)	0·1	0·3	0·8	1·5	2·4	2·8	3·0	2·2	1·2	0·5	0·1	0·1	15·0

Assuming no major change in storage between 1st January, 1947, and 31st December, 1949, the mean difference between annual rainfall and annual run-off was 15·2 inches using the Meteorological Office estimates of rainfall from Table 1. (The local estimates led to a difference of 16·1 inches). The agreement in the annual totals was sufficiently good to justify trust in the monthly values. Comparing those above with the estimates of columns (e), (l), and (q) of Table 5, there was extreme discordance. The values in the Table could not be defended on any physical basis; values of evaporation of 1 inch per month in winter months in Northern Scotland were meteorologically impossible. But if such values were rejected, then

[1] A. J. Bamford. "The Problem of River Floods, and the Relation Between River Height and Rainfall." Abs. in Sess. Notices No. 2, 1933–4, p. 53.

the values of consumption in the preceding columns must be rejected too, in fact some re-examination of the basic assumptions was desirable.

It was possible to start that from Table 5. Considering 1947 as a particular case, column (*a*) stood (although he preferred the Meteorological Office values of rainfall) and column (*b*) stood, as both were based on observation. Column (*e*) was to be re-written as above. Using R, r, E and S to denote rainfall, run-off, evaporation and storage, the simplest balance equation was :

$$S_1 + R = S_2 + r + E$$

or

$$S_1 - S_2 = r + E - R$$

that was to say, the change in storage during any month was the sum of the run-off and evaporation minus rainfall. As all terms on the right-hand side were known, the left could be estimated, and a plot of run-off against storage could be made. The result was chaotic, partly because of variable time lags between occurrence of rain and increase in run-off. There was most semblance of order in summer months when evaporation was greatest, and it might be expected to be greater still in dry-weather periods.,

For those it could be agreed that the greater the storage the greater the rate of run-off, but it was too great a simplification to assume a direct proportionality as was done by the Author. That simple relation between head and flow would hold only for discharge through a fixed resistance, but in practice the resistance increased as the head decreased, because the layer of saturated soil or rock that was transmitting water was getting thinner. As a step forward, but still probably too greatly simplified, it could be said that

$$\text{flow rate} = \text{head/resistance},$$

setting the head as proportional to the storage above some base level, and, as a guess, setting resistance as inversely proportional to the same quantity. Then, in symbols,

$$\frac{dr}{dt} = \alpha\,(S - S^1)^2.$$

It was to be noted that, qualitatively, that gave a range of values of K (as used by the Author) increasing as storage decreased.

As a further assumption, during dry-weather periods the daily rate of evaporation could be taken as constant ($= c$, say), and hence, starting from a storage S_o at time $t = 0$, the simplest balance equation was

$$S = S_o - r - ct,$$

but that assumed that all evaporation was taken from storage, which could not be true, for example, of boggy patches where water was held in a way that did not contribute to the pressure head causing run-off. It seemed reasonable to postulate that only a fraction of the evaporation was withdrawn from storage, and that that fraction would vary during the year,

for though it might be unity in winter it might be very small in summer. If the active balance equation were re-written as:

$$S = S_o - r - \beta^2 ct. \quad (\beta^2 < 1)$$

then
$$\frac{ds}{dt} = -\frac{dr}{dt} - \beta^2 c$$

$$= -\alpha(S - S^1)^2 - \beta^2 c.$$

Then, writing
$$y = (S - S^1)/a, \quad \text{where } a^2 = c/\alpha,$$

$$a\frac{dy}{dt} = -\alpha a^2 y^2 - \beta^2 c$$

$$= -c(y^2 + \beta^2).$$

Therefore
$$\frac{dy}{y^2 + \beta^2} = -\frac{c}{a}dt.$$

Integrating:

$$\frac{1}{\beta}\tan^{-1}\frac{y}{\beta} = -\frac{c}{a}t + \text{const.},$$

or over an interval from t_1 to t_2,

$$\tan^{-1}\frac{y_1}{\beta} - \tan^{-1}\frac{y_2}{\beta} = \beta.\frac{c}{a}(t_2 - t_1).$$

Also
$$\frac{dr}{dt} = cy^2.$$

Those equations were difficult to handle analytically, particularly as β and a were unknown, but by trial it was possible to find values of β that would lead to the same value of α at different times of the year.

Periods in May, August and November, 1947 (doubtful) could be used. From *Fig. 6* the values obtained were:

Month	Period (days)					
May	10	0·17	0·05	0·08	1·46	0·79
August	20	0·025	0·01	0·08	0·56	0·35
November	10	0·35	0·10	0·003	10·80	5·80

By taking a range of values of β from 1·00 down to 0·22 (which was the lowest possible because it represented the area of open water for which $\beta^2 = 0{\cdot}05$, about 1/20th of the catchment), it was found that α must be very close to 0·022, and that the corresponding values of β were :—

May, approximately 0·65 ; August, 0·22 ; November approximately 1·0, that was to say, the evaporation reducing the storage contributing to run-off head was occurring on about 40 per cent of the area in May, about 5 per cent in August (only on the lake) and on all the catchment in November.

The August result was interesting. If correct, it meant that all the August evaporation was drying out the soil without depleting storage and

that at the end of the month the soil moisture deficit would be August evaporation minus August rainfall, in fact about 2 inches. That deficit would have to be made good before any water was available for percolation and there would be a further delay before such percolation reached a zone where it could contribute to the hydrostatic head, meaning that there would be no increase in run-off until at least 2 inches of September rain had fallen. *Fig. 6* showed that to be true qualitatively, for the dotted line implied that rain had fallen early in September but it had been the middle of the month before run-off increased.

In his composite curves the Author had neglected that drying of surface soil as distinct from depletion of storage. It could not be, and must not be, neglected; otherwise the unacceptable estimates, already criticized, in Table 5 and the equally unacceptable values in *Figs 9* and *10* were reached.

Dr Penman said that his comments had been prepared in haste and both analysis and arithmetic might contain errors. It was the principles that were important, however, and they could be stated briefly:

1. The mean monthly values of evaporation for the catchment were known with adequate precision, and any other estimates that were in violent disagreement with them must be suspect.

2. Evaporation could and did take place without drawing on storage, and the fraction of evaporation that could be classified as consumption might vary widely throughout the year.

Mr E. Gold said that he had been doing some work on the run-off in the Thames basin, using the meteorological factor, rainfall, alone. He thought that that could be justified when dealing with monthly or annual values, because over a period of 1 month or 1 year the other important factor, evaporation, was approximately a linear function of the rainfall. If the evaporation and run-off were both linear functions of the rainfall, not necessarily the rainfall of the time of the run-off but including rainfall of previous periods, then there was complete justification for not bringing the evaporation explicitly into the calculations. It was accounted for by the rainfall in a linear formula.

The justification for the linear formula could be seen by plotting the rainfall minus the run-off against the rainfall of the Thames Valley for each month over a period of 40 years. It would be found that in the summer months the points were almost exactly along a straight line. The relation was linear. In the winter months the points were scattered much more about the straight line.

Having said that, he wanted to say quite explicitly that the same procedure could not be applied to daily values; and that being so, how was it that the Author got a close agreement between the peaks on his curve, calculated and actual? He could only suggest that it was because those peaks in run-off had been determined by the rainfall at the time, which came down sufficiently rapidly for the evaporation to be of negligible

importance in the peak period, and that those peaks would be obtained simply by taking account of the rainfall just before the peaks.

The Author had made some allowance for the fact that his factor K was a variable one. Mr Gold suggested that in the equation on p. 15 of the Paper, the Author should write :

$$QE_2 = K_2 QE_1 + R(1 - K_1).$$

The Author had used the same value of K for QE and for R. But if there was storage S and then there was rainfall R, the consumption, instead of being

$$KR + KS,$$

should be $XR + KS$, where X was quite definitely different from K. It had already been stated in the discussion that the factor connecting the rainfall and the consumption would be different from the factor connecting the storage in the ground and the consumption. It was possible that the value of K should increase immediately after the rainfall and then decrease as time went on, and not be continuously in one direction from the time of the rainfall. It would certainly be true of a basin in which the rainfall took some time to get down to the exit.

He had found that curve B of *Fig. 5* could be represented very closely by a formula :

$$K = 1{\cdot}00 - 0{\cdot}3C^{\frac{2}{3}}$$

(He wrote C instead of QE because he wanted to give it a power). But C, the consumption, was given by :

$$C = S - KS.$$

From those two equations

$$C = 0{\cdot}027S^3,$$

or the consumption varied as the cube of the storage expressed in inches. If that relation held, then taking the maximum value of C from the Author's figures as 0·58 the maximum value of the storage would be 2·8 inches.

He felt that that was a matter of some interest, although he would not like it to be thought that he attached too great an importance to the result, in view of his earlier observation that in dealing with daily values the meteorological elements, other than rainfall, could not be neglected.

Mr Julius Kennard suggested that the title of the Paper was misleading, and that it should read " Relation between Daily Rainfall and Flow from Loch Shin ".

There was no doubt that the fact that the uniform drop in run-off after rainfall had ceased was essentially due to the steady lowering of the lake by the water passing over the weir.

On p. 7 the Author had referred to the minimum and maximum flows, and had stated that in the last 100 years the probable maximum flow had been 8,000 to 9,000 cusecs. That was only about one-third of

the maximum flow that would be expected from the catchment, according to the Institution's Report on Floods, were there no reservoir or loch to regulate it. With a water area of 5 per cent, it was obvious to those who practised in that field that the storage reservoir or, in that case, the loch, played a remarkable part in reducing the intensity of flood discharge.

On the other hand, the Author had mentioned the minimum flow of 27 cusecs, which worked out at 0·2 cusecs per 1,000 acres; Mr Kennard thought that was a low flow, and with the loch he would have expected something higher as a minimum flow.

He had examined Table 5 in the Paper, since it was of fundamental importance. What water engineers always desired to know was what run-off to expect in dry periods. He had tried to make certain calculations from the figures in that Table in order to see what results could be deduced, and he had found himself in difficulty.

Taking the driest 6 consecutive months of each of the years 1947, 1948 and 1949 as representing a period of drought in the particular year, the figures were:—

	1947		1948		1949
R:	17·51 inches	R:	22·37 inches	R:	20·75 inches
r:	12·31 inches	r:	15·99 inches	r:	8·85 inches
E:	5·2 inches	E:	6·38 inches	E:	11·90 inches

There appeared to be some relation between the 1947 and 1948 figures, because the rainfall in 1948 had been a little bit higher than in 1947, but the very high loss in the driest 6 consecutive months of 1949, and the very low run-off, seemed incongruous. He was puzzled as to why that had come about.

He had then looked at the matter from another aspect. A method which the water engineer commonly adopted in dealing with the run-off of a particular catchment was to consider the percentage distribution of run-off month by month in a particular year, preferably the driest year if the records were available for that year. For a particular catchment with which he was very familiar, the monthly figures, from January to December, were:

10, 12, 12, 8, 5, 3½, 2, 1, 2, 6, 16½, 22, per cent.

The total for the driest 6 consecutive months was 19½ per cent. It was a curious thing that in a large number of gaugings of various watersheds that he had had occasion to examine, the run-off in the driest 6 consecutive months had been approximately 20 per cent of the annual run-off, and that if there had been any substantial variation from that 20 per cent, there had been some special reason for it.

Taking the figures that could be extracted from Table 5, in 1947 the 6 months showed a total of 36½ per cent, in 1948, 28½ per cent, and in 1949,

$19\frac{1}{2}$ per cent. Once again, he was at a loss to explain why there should be such a large variation in the percentage run-off in the driest 6 consecutive months.

In looking at *Fig. 1*, a plan of the River Shin catchment area, he noticed that the River Tirry seemed to drain a very large part of the area—approximately one-third, according to the map. Could the Author place any reliance on the figures that he had set out in the Paper in estimating or forecasting what the daily, or even monthly, run-off would be on the River Tirry without the stabilizing or regulating effect of Loch Shin ?

*** **Mr G. R. Hoffman** said that the Author had suggestde a new method by which run-off could be calculated from rainfall. Such a complex subject could not be dealt with without making numerous simplifying assumptions and those made by the Author in dealing with evaporation were the easiest to criticize ; yet that criticism did not affect the basic assumption made : that the consumption from a catchment was a function of the storage.

With reference to the justification for that assumption, for a catchment from which there was no evaporation, Mr Hoffman assumed the general relation between run-off Q and storage R given in equation (1) :

$$Q = -\frac{dR}{dt} = aR^n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which a and n were constants.

Differentiation gave :

$$\frac{dQ}{dt} = -na^{\frac{1}{n}} Q^{2-\frac{1}{n}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Equation (2) showed that such an assumed relation between Q and R implied that for a given value of Q the slope of the recession curve for no rain was constant. It was on the validity of that assumption that the method relied.

If equation (2) was correct in form, values for a and n could be calculated from the observed recession curve of flow of the river. It was probable that the connexion between Q and R would change with Q, but if a and n were found to be reasonably constant over fair ranges of Q, then their definition as constant was justified.

For the Loch Shin area, $n = 2{\cdot}32$ over the range $QE = 0{\cdot}02$ to $0{\cdot}10$ inch per day, while for a tank emptying over a weir of the same characteristics as that at the exit from Loch Shin, $n = 2{\cdot}00$.

Integration of equation (1) and substitution of Q for R gave

$$Q_t = \left[Q_o^{\frac{1-n}{n}} + a^{\frac{1}{n}}(1-n)t \right]^{\frac{n}{1-n}} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

*** This and the following contribution were submitted in writing.—SEC. I.C.E.

or for the case when $n = 1$, suggested by Mr Lacey :—

$$Q_t = Q_o e^{-at} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

Equation (3), and of course (3a), were exact over the range for which a and n could be assumed constant. Further elaboration of equation (9) to fit conditions in the catchment under consideration might lead to a greater range of application of the corresponding equation (3), and so to greater accuracy in the final answer. The Author, in putting $n = 1$, was over-simplifying the problem; the consequent continuous variation of K introduced algebraical errors which, although they were not cumulative, might have an appreciable effect over short intervals.

If additional rain fell over a time interval $0 - T$, and were assumed to fall instantaneously at $t = 0$, increasing the storage to R'_o, where $R'_o = R_o + r$:

from equation (1),

$$Q'_o = aR'^{\,n}_o = Q_o\left[1 + n\frac{r}{R_o} + \frac{n(n-1)}{2\,!}\left(\frac{r}{R_o}\right)^2 + \cdots\right] \qquad (4)$$

If terms in $\left(\frac{r}{R_o}\right)^2$ and higher powers could be neglected, that reduced to :—

$$Q_o^{\frac{1}{}} = Q_o + n\,r\,a^{\frac{1}{n}}Q_o^{1-\frac{1}{n}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4a)$$

and Q_t was found by substitution of Q_o' in equation (3).

The assumption that $\left(\frac{r}{R_o}\right)^2$ etc. could be neglected implied that the bigger r was, compared with R_o, the smaller the time interval T between rain-fall observations had to be for equation (4a) to be used. Otherwise more terms in the series in equation (4) had to be taken into account.

It was to be noted that $r = iT$ where i was the average intensity of rainfall over the whole catchment: thus the smaller the catchment area the greater the possible value for r for a given T. In addition the smaller the catchment the smaller the possible values for R. Therefore it could be seen that it was that interval T which in practice restricted the application of the method to large catchments, or those having a high proportion of storage, since rainfall measurements were rarely taken at shorter intervals than 1 day.

Mr Hoffman suggested that his generalization of the Author's method for a catchment with no evaporation could be extended to include evaporation. It remained to be shown whether the assumption of equation (1) was justified, and also whether the introduction of evaporation made the solution of the equations too laborious for practical purposes.

However, the general method outlined could be applied to the prediction of flood flows for a catchment, given the average rainfall intensity—time

curve, on the assumption that evaporation losses were small compared with discharges.

The Interim Report of the Institution Committee on Floods had recommended the probably "normal" and "catastrophic" flood flows in cusecs per thousand acres to be expected from a given area of catchment. That was based on observed flood flows from a number of catchment areas, but necessarily neglected variations due to differences in vegetation, slope, resistance to flow off the ground, etc., from one catchment to another.

The Interim Report had also assumed that extreme floods followed floods of moderate intensity and an initial run-off of 50 cusecs per 1,000 acres had been taken. That could be caused by continuous rain at an average intensity of 0·05 inches per hour, and it might be assumed to imply that (in Great Britain) the catchment was in a saturated condition.

If, instead of specifying intensity of run-off as a function of catchment area, the rainfall intensity and duration (which were independent of the type of catchment) were given as functions of the catchment area, the Author's method could be applied to calculate the flood discharge from any catchment for which the recession curve at high flows for no rain was known: the constants a and n for the catchment could be found from the observed recession curve at high flows, due regard being paid to the time interval T applicable. Flood flow calculated in that way should be a fair guide to what would happen in practice, since use of the recession curve for no rain automatically took account of the nature of the catchment.

Mr R. C. S. Walters asked if the Author, or others, had tried the method on any other areas.

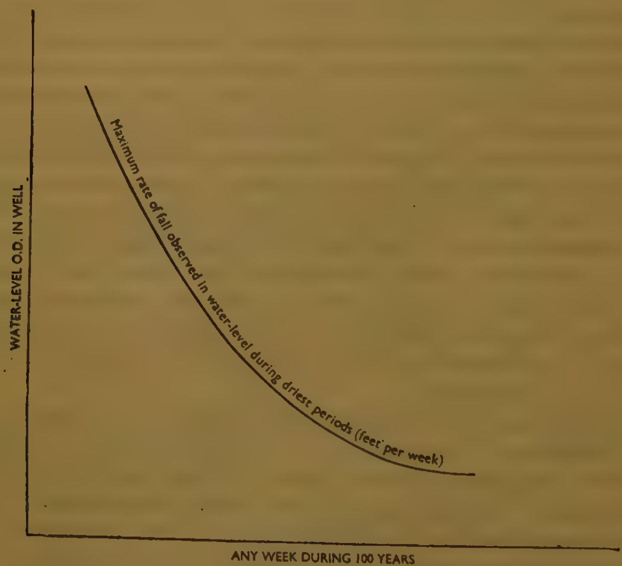
Mr Walters was attracted to the method because of its analogy to Mr Halton Thompson's investigation into the "ultimate percolation" into a well.[1] For that, the first step was to build up a "dry weather depletion curve." That was constructed from the maximum rates of depletion of water level in the well for various levels, observed for many years (*Fig. 16*). The maximum rate of depletion would of course be that obtained in periods of drought, just as suggested by the Author. When rain fell, the depletion curve was altered and, hence, through a mathematical process, the amount of rain entering the underground system could be estimated, and compared with the estimated rainfall on the gathering ground, the "loss" being the difference.

There might be many points of detail open to criticism in the Author's method, but the Author himself had pointed those out quite fairly. Thus, an American publication had stated that for a particular area, it had been found that one rain gauge per 100 square miles on a gathering ground of 500 square miles (that was five gauges in all), would give an error of plus

[1] D. Halton Thomson, "Hydrological Conditions in the Chalk at Compton, W. Sussex". Trans. Instn Water Engrs, vol 26, p. 228 (1921) and vol. 36, p. 176 (1931). "A 100-Years Record of Rainfall and Water Levels in the Chalk at Chilgrove, W. Sussex." Trans. Instn Water Engrs, vol. 43, p. 154 (1938).

or minus 15 per cent of the average rainfall. The same number on 8,000 square miles (eighty gauges), only 6 per cent. It was probable that the isoheyetal maps of the Meteorological Office in Great Britain, in a small area such as the Shin, would give greater accuracy than the American example, but in any event the behaviour of the particular rain gauge observed for the 3 years was so correlated with the discharge that whatever the error in the loss might be, it should still be possible to make an approximate estimate of the maximum and minimum flows through the relative estimates of the long-term rainfall.

Fig. 16



He welcomed any method of determining those, based on 2 or 3 years gauging.

The Author, in reply, thanked Mr Lacey for his suggestion that the tabulation method used in the Paper could be replaced by a graphical method using logarithmic paper. The Author had thought that a step-by-step method would be the simplest way to develop his ideas, but he now saw that Mr Lacey's suggestion was more logical mathematically and would also save much time and tedious work.

The point raised by Mr McClean, that the morning reading of the staff gauge gave too high a figure for the flow, was interesting. The Author had observed the same phenomenon in Western Canada, where large diurnal variations in stream flow occurred from icefields in the early summer. In the late summer the feature became less marked after the surface " soft "

ice had been melted off. In reply to Mr McClean's question, the elevation of Loch Shin was given as 270 feet O.D. The zero of the stage-discharge curve for the current meter gauging site was 243 feet O.D. The logarithmic plot of the stage-discharge curve was linear for all discharges, and that had been confirmed by model experiments on the Loch Shin Weir carried out at University College, Dundee.

The Author hoped that by putting as many of the cards as he could muster on the table, as Mr Wolf had so generously pointed out, a large number of uncertainties inherent in the treatment of rainfall, run-off, evaporation, and other losses might be cleared up by public discussion. He agreed that in the first part of his analysis he had attempted to derive the consumption curves by a logical process of reasoning, but he admitted that in the second part, that is in subtracting a quantity approximate to the losses to derive the run-off, he had used purely empirical methods. If he had known at the time of Dr Penman's work on evaporation, he might have avoided that necessity.

The Author agreed fully with Dr Glasspoole and Dr Penman that the chief weakness of the Paper was in the unsatisfactory treatment of the evaporation. In fact the Author felt that that was the main weakness in every attempt to correlate rainfall and run-off that he had come across. He felt that great benefit would accrue to engineers by the interest shown in the subject of evaporation by Dr Glasspoole and the Meteorological Office, as well as by Dr Penman. It should be noted that not long ago the Meteorological Office had been unwilling to provide evidence on the question of evaporation. The Author felt that the discrepancies in his curves in *Figs 11*, *12*, and *13* were due partly to grouping all losses under the term "Evaporation" and partly to the method of recording precipitation, which, while probably satisfactory for meteorologists, was not of much use to engineers who were interested in the yield. Anyone who had been in mountain country was conversant with the view of fresh snow on the hill-tops after a rainstorm had passed in the valley. The precipitation which was recorded as its equivalent in water was partly ready for immediate run-off and partly withheld as snow. Fortunately in the British Isles all "banked" snow could be assessed in annual estimates of the yield (even if that were not so in the case of the monthly and daily figures), but in cold or mountainous country, where the snowline was permanent, that part of the precipitation which fell above the snowline might not appear in the yield for years. It would be interesting to have Dr Glasspoole's views on how meteorologists could assist engineers in the separation of rain from any part "banked" in the form of snow.

The Author was grateful for Dr Penman's observation on the problem of the water held within the soil (but not available for run-off) which, in dry weather, was depleted by evaporation and transpiration and which had to be restored from subsequent rain. The Author felt that his curves, which showed a high degree of error in early September, went some way

towards revealing the presence of that feature. He agreed, however, that that should not be classed directly under the heading of evaporation.

The Author thanked Dr Penman for his very clear and penetrating review and he asked if Dr Penman might perhaps publish daily as well as mean monthly evaporation tables and, if it were possible, classify mean daily evaporation in accordance with type of climate rather than by calendar months. If those figures had been known, the Author could have used them to derive river flow without the necessity of first making his own assessment of the daily evaporation. He hoped that a considerable improvement in accuracy would result.

With regard to Mr Gold's remarks, the Author agreed that the hydrograph of the peaks of floods was largely exclusive of evaporation, and that, therefore, a direct relationship between rainfall and run-off was easier to arrive at. This, however, had to be qualified by the fact that, at times of peaks, partial retention on the catchment of the precipitation in the form of snow might, on occasion, still make results confusing.

With regard to Mr Kennard's remarks on the minimum flow, the Author was satisfied that in August 1947 the flow down the Shin was as stated, and also that as far as could be seen, it was certainly the lowest since 1927, and might even be considered an all-time low.

The figures used by Mr Kennard in Table 5 of the Paper were those of the rainfall and run-off " as measured " and were, therefore, independent of any theory developed by the Author. Allowances for variation in storage had to be made, and the figures, which were considered reliable, merely represented the facts of the River Shin Catchment as far as could be ascertained.

In regard to the question of the application of the method to the Tirry, the Author had found that the Tirry was so flashy that some floods were known to rise and fall in a matter of a few hours. Daily rainfall records, therefore, could not be used to assist in tracing the hydrographs of the peaks of those floods, and the Author was fortunate in finding that the Shin, in which the floods appeared about 1 to 2 days after the commencement of the rainfall, was so adapted that he could use the available data in the way he had done.

The Author agreed with Mr Hoffman that he had tended to oversimplify the very complex nature of the hydrologic cycle. He had done so in an attempt to show that a first approximation to the run-off might be made from the rainfall without undue complication. If increased accuracy were required, then it would be necessary to introduce the additional factors referred to by Dr Penman. He was pleased to learn that Mr Hoffman's remarks appeared to confirm that the reasoning in the Paper had some logical mathematical basis, and that it might, therefore, be possible to improve the method considerably by taking further factors into account.

The Author thanked Mr Walters for bringing attention to Mr D. Halton Thompson's previous work on the " Dry Weather Depletion Curve." The

Author had made efforts to find some other areas where the method could be tried but he had not been able to find one with sufficient data for a day-to-day analysis. He had found, for instance, that either run-off or rainfall data, or both, were generally insufficient for the purpose in the case of suitable catchments, and that considerable time would be necessary to prepare the figures for handling. He hoped, however, that perhaps someone else interested might be able to do so.

STRUCTURAL AND BUILDING ENGINEERING DIVISION MEETING

27 November, 1951

Professor ALFRED JOHN SUTTON PIPPARD, M.B.E., D.Sc., M.I.C.E., Chairman of the Division, in the Chair.

The following Paper was presented for discussion and, on the motion of the Chairman, the thanks of the Division were accorded to the Authors.

Structural Paper No. 29

"The Rapid Calculation of the Plastic Collapse Load for a Framed Structure"

by

Bernard George Neal, M.A., Ph.D., and Paul Southworth Symonds, Ph.D.

SYNOPSIS

A framed structure of ductile material, such as mild steel, collapses when the applied loads are increased to such values that a sufficient number of plastic hinges are formed to transform the structure into a mechanism, which can then continue to deform under constant load. For complicated frames the determination of the positions of the plastic hinges, and thus of the collapse load, may become very lengthy by existing methods. The Paper presents a new method of determining the plastic-hinge positions and the corresponding value of the collapse load, which is considerably shorter than methods described previously.

INTRODUCTION

PLASTIC design methods for rigidly jointed steel frames have been advocated by Professor J. F. Baker[1] in a recent Paper to the Institution. Such design methods are applicable to framed structures of ductile material whose members may be assumed to possess the following properties :—

(1) If the curvature increases indefinitely at any particular cross-section, the bending moment tends to a limiting value at this cross-section. This limiting value is termed the fully plastic moment, and its magnitude is independent of the previous history of loading.

[1] The references are given on p. 71.

(2) An increase of curvature is always accompanied by an increase of bending moment of the same sign, except when the fully plastic moment is attained.

The behaviour of mild-steel beams conforms closely to these assumptions, and so the plastic design methods are very suitable for mild-steel frames.

If the fully plastic moment is attained at a particular cross-section of a member, the curvature at this cross-section is indefinitely large. It is then possible for a finite change of slope to occur over an indefinitely small length of the member at this cross-section. When this happens, a plastic hinge is said to have formed. Rotation of a plastic hinge can only occur when resisted by the fully plastic moment, for if the bending moment is reduced elastic unloading occurs, and the hinge rotation remains constant.

Plastic design methods are based on the load at which a framed structure of ductile material collapses. For a structure of this kind, collapse occurs when, under the applied loads, a sufficient number of plastic hinges are formed to transform the structure into a mechanism, the various individual loads being assumed to remain in constant ratio with one another. If the structure has n redundancies, it will always be possible to find one or more ways in which $n + 1$ plastic hinges can be located in the frame so as to produce a mechanism with one degree of freedom. Since plastic hinges only form when the bending moment has reached the fully plastic moment, the choice of $n + 1$ plastic hinges amounts to specifying the values of $n + 1$ bending moments. In such a case, the value of the applied load, which is taken to be specified by a single parameter, can be calculated from the equations of equilibrium. From these equations the bending-moment distribution throughout the entire frame can also be calculated. If any assumed mechanism of this type leads to a distribution of bending moments which do not exceed the fully plastic value anywhere in the frame, then it can be shown that this mechanism is the actual one which will form when the frame collapses, and the corresponding load is then the collapse load.[2, 3, 5] Thus by a trial and error process, collapse loads can be calculated by assuming various collapse mechanisms and calculating the corresponding bending-moment distributions.[1, 4]

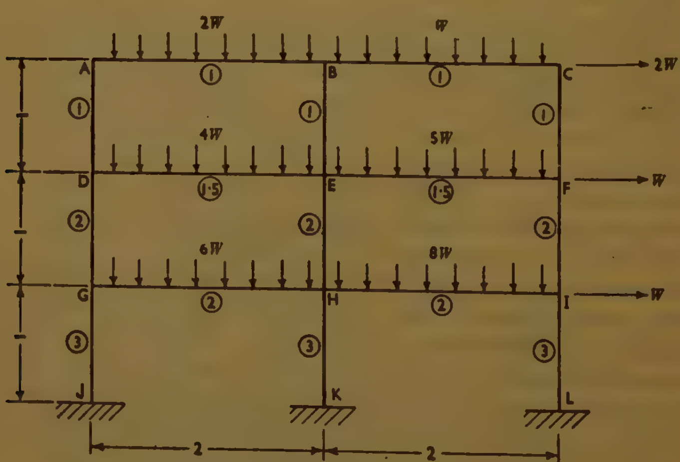
However, in complex frames it is possible, and in fact quite likely (from a number of examples which have been calculated), that the collapse mechanism will involve fewer than $n + 1$ plastic hinges. When this happens, the collapse is said to be partial. In such cases the trial-and-error approach is greatly complicated, for the bending-moment distribution throughout the frame cannot be determined solely from the equations of equilibrium. It becomes necessary to investigate whether any bending-moment distribution exists, consistent with the equations of equilibrium, such that the fully plastic moment is not exceeded anywhere in the frame. Such an investigation may be very tedious. Moreover, in many cases there may be a number of possible partial-collapse mechanisms which have to be investigated.

The Paper describes a new method for determining the actual collapse mechanism and the corresponding collapse load. Instead of becoming more complicated when the collapse mechanism involves fewer than $n + 1$ hinges, this method is, if anything, simpler in such cases. The method is based on the known fact that if any arbitrary collapse mechanism is selected, the corresponding load is either greater than or equal to the actual collapse load.[2, 3, 5] The procedure consists essentially of combining the various local-collapse mechanisms, and adjusting rotations of joints, until a mechanism is found such that any further permissible combination or joint rotation produces an increase in the collapse load. This mechanism will then be the actual collapse mechanism, and the corresponding load will be the actual collapse load. The examination of the various local-collapse mechanisms and their combinations, as well as the effect of joint rotations, is made extremely simple by the use of the Principle of Virtual Work,[5] which allows the collapse load for any given mechanism to be written down by inspection. By this Principle the various combinations and adjustments needed to reduce the collapse load corresponding to any possible mechanism to the lowest possible value proceed very quickly. The procedure will be illustrated by a numerical example.

Illustrative Example

The rigidly jointed rectangular frame shown in *Fig. 1* will be used as an illustrative example. The six beams each carry a uniformly distri-

Fig. 1



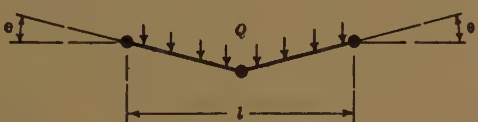
Loads and Proportions of Frame

buted load of magnitude shown in the figure, and in addition there are side loads at each beam level. To shorten the numerical work, each storey is

taken to be of unit height, and each beam has a span of 2 units. The fully plastic moments for each member are indicated by the ringed numbers in the figure, the fully plastic moments of the top beams and stanchions being taken as unity. The loads are all specified in terms of a single parameter W, and the problem is to determine the value of W at which plastic collapse would occur, and the corresponding collapse mechanism.

The analysis is commenced by considering the various cases of local collapse. Considering, for instance, a uniform beam of length l, whose fully plastic moment is M_p, which is subjected to a uniformly distributed load Q, collapse of this beam will occur when plastic hinges form at the ends and centre of the beam, as shown in *Fig. 2*.

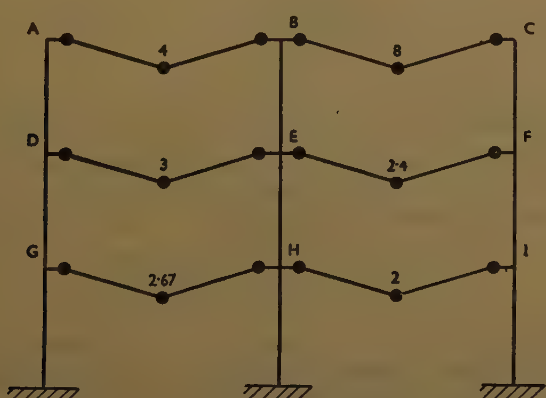
Fig. 2



LOCAL COLLAPSE OF BEAM

If the plastic hinges at the ends rotate through an angle θ, the central plastic hinge will rotate through an angle 2θ. The Principle of Virtual Work can be applied to this system in the simple form that the virtual work done by the loads on any system of small displacements corresponding to motion as a mechanism is equal to the virtual work done by the fully plastic moments on the plastic-hinge rotations. Since the central displacement of the beam in *Fig. 2* is $\frac{1}{2}l\theta$, the virtual work done by the loads

Fig. 3



VALUES OF W FOR LOCAL COLLAPSE OF BEAM

is $\frac{1}{4}Ql\theta$. The virtual work at each plastic hinge must be positive, so that the virtual work absorbed by the plastic hinges is $4M_p\theta$. Thus:

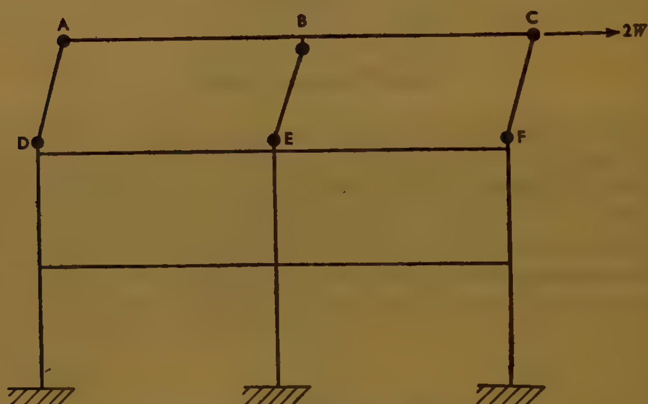
$$\frac{1}{4}Ql\theta = 4M_p\theta$$

$$Q = \frac{16M_p}{l} \quad \ldots \ldots \ldots \quad (1)$$

The six beam failures of this type are represented in *Fig. 3*, together with the corresponding values of W calculated from equation (1). The smallest value of W found in this way is $W = 2$ for beam HI. Thus the actual value of W at collapse cannot exceed 2.

The other type of local collapse occurs when one storey of the frame fails in side-sway. The three possible failures of this type are represented in *Figs 4, 5,* and *6*, the corresponding values of W, as calculated from the

Fig. 4



Local Collapse of Top Storey
$W = 3$

Principle of Virtual Work, being given in the Figures. For instance, considering the local collapse of the bottom storey illustrated in *Fig. 6*, if each stanchion rotates through an angle θ, each side load moves through a distance θ, and each plastic hinge rotates through an angle θ. Thus:

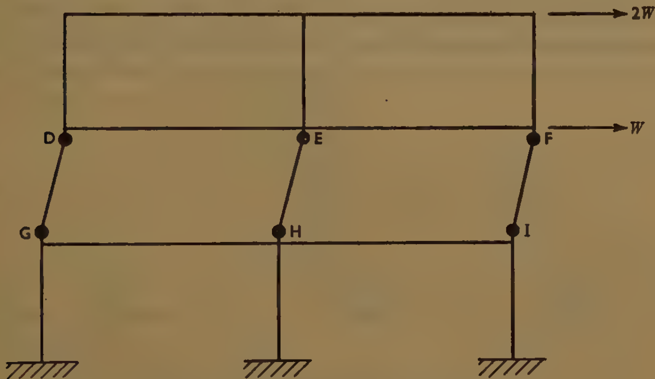
$$(2W + W + W)\theta = 6 \times 3\theta$$

$$W = 4{\cdot}5$$

In all, nine local collapse mechanisms are illustrated in *Figs 3–6*. These mechanisms can be altered by combination with rotations of the seven joints B, D, E, F, G, H, and I (*Fig. 1*), and by combination with one another. The problem is to find the particular combination which gives the smallest value of W.

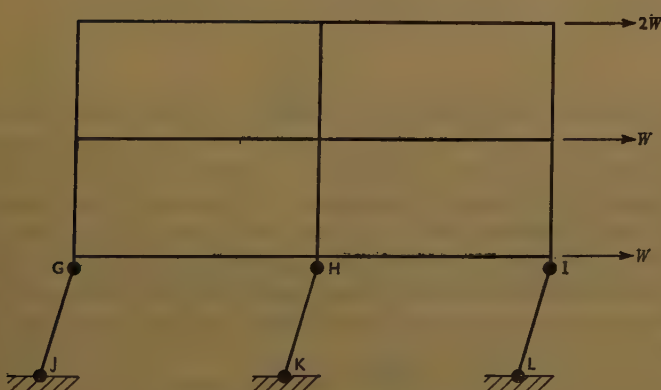
The first step is to investigate the possible combinations of the side-sway collapses. For instance, the superposition of the side-sway mechanisms for the two upper storeys, if done arbitrarily, would result in two plastic

Fig. 5



Local Collapse of Middle Storey
$W = 4$

Fig. 6

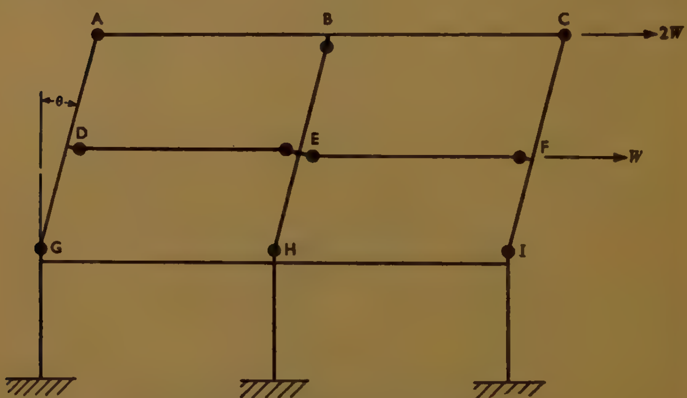


Local Collapse of Bottom Storey
$W = 4{\cdot}5$

hinges at the joint D. However, by making the inclination of the stanchions AD and DG equal, and rotating the joint D, these two plastic hinges can be replaced by a single hinge in the beam DE. This decreases W, since the virtual work absorbed in this single hinge is $1{\cdot}5\theta$, as compared

with $(2+1)\theta = 3\theta$ for the two hinges in the stanchions. Similarly, a rotation of the joint F is made when combining these mechanisms. The final result of this superposition is shown in *Fig. 7*. It will be seen that joint E is rotated so as to replace the plastic hinges in the stanchions BE and EH by plastic hinges in the beams DE and EF. This actually leaves the virtual work absorbed by the plastic hinges unchanged. The reason for this preference will become clear when superposition of the beam mechanisms is considered. The value of W is 3, which is no improvement over the value obtained for side-sway of the top storey only.

Fig. 7



COLLAPSE OF UPPER TWO STOREYS
$W = 3$

If the lower two storeys are permitted to sway, the top storey remaining rigid, it is found that $W = 3{\cdot}29$. However, if all three storeys sway, as shown in *Fig. 8*, the value of W is $2{\cdot}89$, a slight improvement over the value obtained for the upper storey alone. In this case it is advantageous to rotate the joint H in order to produce plastic hinges in the beams GH and HI rather than in the stanchions EH and HK, for this rotation reduces the virtual work absorbed in the plastic hinges at this joint from 5θ to 4θ. For reference, the virtual-work equation for the mechanism shown in *Fig. 8* is:

$$9W\theta = 26\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

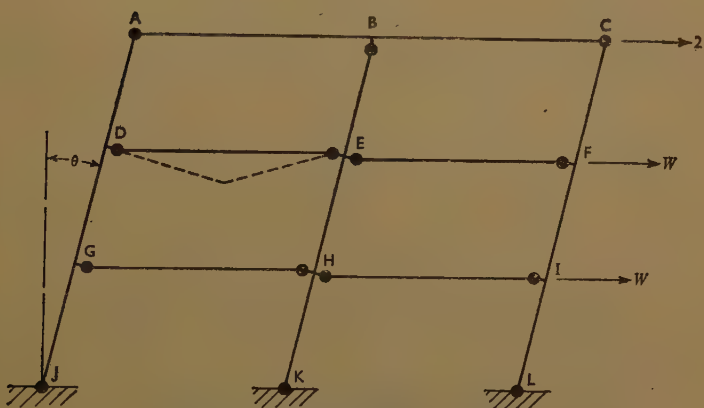
The next step is to combine the local collapses of the beams with one of the side-sway failures in an attempt to lower W. In making these combinations it will be assumed that the plastic hinge under the distributed load remains at the centre of each beam. This will not generally be the case. However, it is simplest to make this assumption in the first part of the analysis and to find the mechanism yielding the lowest value of W consistent with this assumption. Then each beam can be considered

in turn, and the correct location of the plastic hinge under each distributed load can be determined. This usually results in a further lowering of W, which is less than 1 per cent for each distributed load.

The obvious choice of side-sway failure with which to combine the beam failures is the one which gives the lowest value of W, as shown in *Fig. 8*. Consider, for example, the combination of the failure of the beam DE with this side-sway mechanism (as indicated by the dotted lines in *Fig. 8*). For local collapse of the beam DE in the manner indicated in *Fig. 2*, with plastic-hinge rotations θ at each end of the beam and 2θ at the centre, the virtual work equation is :

$$2W\theta = 4\times 1{\cdot}5\theta = 6\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$
$$W = 3$$

Fig. 8



COLLAPSE OF ALL THREE STOREYS
$W = 2{\cdot}89$

The virtual work equation for the side-sway failure of *Fig. 8* has already been given as equation (2). When these two mechanisms are combined, as indicated in *Fig. 8*, the plastic hinge at D disappears. The right-hand side of each of equations (2) and (3) included a term $1{\cdot}5\theta$ for the virtual work absorbed in the plastic hinge at D, so that the combination of these two mechanisms reduces the virtual work absorbed in all the plastic hinges by 3θ. However, the virtual work done by the loads in each mechanism remains unchanged by the combination, so that, referring to equations (2) and (3), the virtual work equation for the combination is :

$$(9+2)W\theta = (26+6-3)\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$
$$W = 2{\cdot}64$$

This value of W is less than the value for each of the mechanisms which were combined, because of the removal of the plastic hinge at D. This

confirms the choice of the particular sidesway mechanism of *Fig. 8.* This mechanism, as well as possessing the smallest value of W for side-sway, may be combined with local collapses of all the beams except BC to remove the plastic hinges at the left-hand ends of these beams. This may prove to be advantageous, as in the case of the beam DE.

On investigation, it is found that W is reduced by superposing beam failures for EF, GH, and HI, but not for AB. The successive values of W obtained in this way are :

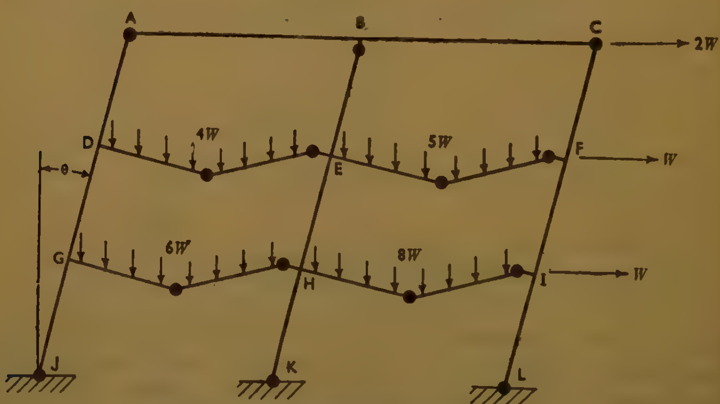
Beam EF . . . . . . $W = \dfrac{32}{13{\cdot}5} = 2{\cdot}37$

Beam GH . . . . . . $W = \dfrac{36}{16{\cdot}5} = 2{\cdot}18$

Beam HI . . . . . . $W = \dfrac{40}{20{\cdot}5} = 1{\cdot}95$

Thus by this process a mechanism is obtained which gives a value of W slightly lower than the value 2 found for local collapse of the beam HI

Fig. 9



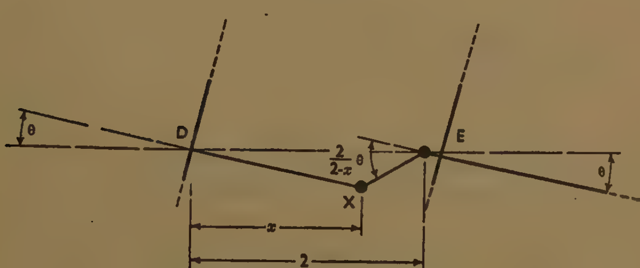
COMBINED COLLAPSE MECHANISM
$W = 1{\cdot}95$

alone. It is a simple matter to check that no further reduction can be achieved by superposing any joint rotations or any of the local side-sway collapse mechanisms ; it can therefore be concluded that the resulting mechanism, shown in *Fig. 9*, represents the actual collapse mechanism, subject only to the proviso that, since plastic hinges can occur anywhere in the span of a beam subjected to a distributed load, some further small reduction in W can be effected by calculating the optimum positions for the hinges in the beams.

Considering, for example, the beam DE, instead of assuming a plastic hinge at the centre of this member, it is supposed that the hinge actually occurs at X, at a distance x from D, as shown in *Fig. 10.*

The vertical deflexion at this hinge is then $x\theta$, so that the average deflexion of the load $4W$ is $\frac{1}{2}x\theta$. The virtual work done by this load is therefore $2Wx\theta$. From the geometry of the figure it can be seen that the rotation of each of the plastic hinges at X and at E is $\dfrac{2}{2-x}\,\theta$, so that the the virtual work absorbed in these hinges is $\dfrac{6}{2-x}\,\theta$, the fully plastic moment being 1·5. The virtual work equation for the entire frame,

Fig. 10



VARIABLE PLASTIC-HINGE POSITION

aking into account the variable position of the plastic hinge in DE, is hus:

$$W\theta(18{\cdot}5 + 2x) = \left[34 + \frac{6}{2-x}\right]\theta$$

$$W = \frac{74 - 34x}{37 - 14{\cdot}5x - 2x^2}$$

he value of x which minimizes this expression for W is readily found to e 0·73, the corresponding value of W being 1·94.

Taking into account this value of x, beam EF may be considered next. ssuming a plastic hinge at a distance y from E, a procedure similar to the bove gives $y = 0{\cdot}89$ and $W = 1{\cdot}94$. Since this plastic hinge is located ery near the centre of the beam, there is no change (to three significant gures) in W.

For beam GH the optimum position for the plastic hinge is found to e at a distance 0·83 from G, giving $W = 1{\cdot}93$; and for beam HI the lastic hinge is located at a distance 0·98 from H, leaving W unchanged o three significant figures).

Although it was found that the value of W was increased by introducing plastic hinge at the centre of the beam AB, it is found that a reduction

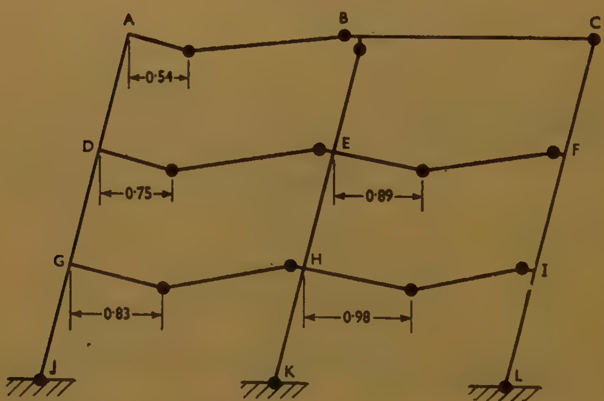
in W can be achieved if this plastic hinge occupies some other position. The least value of W is found to be 1·91, which occurs when the plastic hinge is located at a distance 0·54 from A. There is found to be no advantage in introducing a plastic hinge anywhere in the span BC.

After each minimization of W, the magnitudes of the virtual work terms alter slightly, but on returning to beam DE it is found that the optimum value of x is only altered from 0·73 to 0·75, so that recalculations are superfluous.

The actual collapse mechanism as obtained by the above calculations is shown in *Fig. 11.*

It should be pointed out that the units in which W is measured are

Fig. 11



ACTUAL COLLAPSE MECHANISM
$W = 1{\cdot}91$

consistent with taking the height of each storey, say l, as the unit of length, and the fully plastic moment of the uppermost beams and stanchions, say M_p, as the unit of bending moment. Thus the unit of load is $\frac{M_p}{l}$, so that:

$$W = 1{\cdot}91 \frac{M_p}{l}$$

Therefore, $M_p = 0{\cdot}52Wl$, so that, for given values of W and l, suitable sections for the members of the frame could be selected.

GENERAL DISCUSSION OF METHOD

Underlying the present method is the statement that the collapse load corresponding to any arbitrarily selected mechanism must be at least as

great as the actual collapse load of the frame. To justify this statement it is merely necessary to point out that the difference between a correct collapse mechanism and an incorrect one for a given frame is that in the latter the bending moment exceeds the permissible fully plastic moment somewhere in the frame, whilst in the actual collapse mechanism the fully plastic moment is not exceeded anywhere. But clearly an incorrect mechanism for the actual frame is the correct collapse mechanism for a hypothetical frame, which is similar to the original frame except that it has been strengthened at various sections so that the fully plastic moment of no member is exceeded. Since strengthening a given frame anywhere either increases its collapse load or leaves it unchanged, it is apparent that the collapse load found by considering any arbitrary mechanism must be either the correct collapse load, or an incorrect value which is too high. This argument is due to Greenberg and Prager.[5]

In applying the technique suggested in the Paper it is essential that combinations of all the independent "mechanisms" should be considered. The term "mechanism" is here taken to include rotations of joints as well as the local-collapse mechanisms illustrated in *Figs 3, 4, 5,* and *6*. To ensure that all are considered it is often helpful to bear in mind that the number of independent mechanisms is just the number of independent equations of equilibrium among all the bending moments at sections where fully plastic moments can occur. In general, fully plastic moments can occur at the end sections of all members, at points of application of concentrated loads, and also at one section in a span subjected to a distributed load. Thus for the frame considered in the Paper there are three possible plastic-hinge positions in each of the six beams, and two possible plastic-hinge positions in each of the nine stanchions. This apparently gives thirty-six possible plastic-hinge positions but, since the bending moments in the beams and stanchions meeting at A and C must be equal, the number of possible plastic-hinge positions is actually thirty-four. Since the frame has eighteen redundancies, there must therefore be sixteen independent equations of equilibrium, and therefore sixteen independent mechanisms.

For a rectangular framework of the kind considered in the Paper it is easy to distinguish these sixteen independent mechanisms. Here there are six beams, each of which has its own local collapse mechanism of the type shown in *Fig. 3*, and there are three independent side-sway mechanisms shown in *Figs 4, 5,* and *6*. These nine independent mechanisms, together with the possible rotations at the seven joints where three or more members are connected, correspond to the sixteen independent equations of equilibrium of this structure. In other structures, such as pitched-roof portals, it may be less obvious how many independent mechanisms exist. It will always be easy to ensure that all are considered by counting up the total number of bending moments and subtracting the number of redundancies. It will also be useful to note the two classes of local collapse; the *beam* local-collapse mechanisms (as in *Fig. 3*); and the *frame* local-

collapse mechanisms (as in *Figs 4, 5,* and *6*). These will occur in any frame and, together with the rotations at joints where three or more members meet, constitute the independent mechanisms which combine to produce the actual collapse mechanism.

An attractive feature of the method described in the Paper is that it is unnecessary to establish any sign conventions for the bending moments. In the virtual-work equations only fully plastic moments appear, and the virtual work done by each of these on the small plastic-hinge rotations in any arbitrary mechanism is always positive. A further feature is that there is no need to write down the equilibrium equations in order to obtain the solution. However, it would probably be prudent to check the solution by substituting the corresponding fully plastic moments in the equations of equilibrium and determining the bending-moment distribution for the entire frame. If the fully plastic moment is not exceeded anywhere in the frame, the solution is correct.

Comparison with other Methods

It is interesting to contrast the method with the other available methods for determining collapse loads. To date, the most frequently used method has been that described by Professor J. F. Baker.[1, 4] This method is to guess a complete mechanism, and then determine the bending-moment distribution in the entire frame by means of the equations of equilibrium. If the guess is incorrect, a fresh guess is made in which plastic hinges are placed at some or all of the positions where the fully plastic moment is exceeded. In this way the correct collapse mechanism is eventually found. The advantage of the method described in the Paper is that the equations of equilibrium need only be solved once as a check, and that there is no difficulty in dealing with cases of partial collapse, as discussed in the Introduction. Another available method involves the solution of the set of simultaneous inequalities which express the fact that the fully plastic moment is not exceeded anywhere in the frame.[6] This method, whilst presenting no intrinsic difficulties, becomes very lengthy for complex frames, and it has the further disadvantage that it does not enable a picture of the physical behaviour to be evolved until the calculations are complete.

A method for determining both upper and lower bounds on the collapse load has been given by Greenberg and Prager.[5] The upper bound is obtained by assuming any mechanism and applying the principle of virtual work. A lower bound is then obtained by calculating the bending-moment distribution throughout the structure and reducing all the bending moments and the load in proportion until the fully plastic moment is not exceeded anywhere. The underlying idea of this method is to narrow the bounds until a solution sufficiently accurate for all practical purposes is

found. Usually, however, the bounds remain fairly widely separated until the correct solution is obtained.

Recently, an interesting technique for obtaining a series of converging upper and lower bounds on the collapse load was given by Heyman and Nachbar.[7] This consists of placing plastic hinges at every possible section in a frame, thus producing a mechanism with many degrees of freedom, from which an upper bound on the collapse load can be calculated. A systematic procedure is available for reducing the number of degrees of freedom and thus lowering this upper bound. Simultaneously, a lower bound on the collapse load is found by constructing a bending-moment distribution which satisfies the equations of equilibrium and does not exceed the fully plastic moment anywhere in the frame. The analysis proceeds until the gap between the upper and lower bounds on the collapse load is sufficiently small to enable an approximate result to be quoted. This method possesses the great advantage that it is reasonably practicable for complex frames. However, it precludes to some extent the building up of a physical picture as the analysis proceeds.

From this discussion, it is apparent that the method desciibed in the Paper draws upon several features of these other methods. Recognition of the fact that the actual collapse mechanism can always be obtained by superposing the independent local collapse mechanisms has enabled these features to be welded into a method which is considerably swifter than methods which have been described previously.

References

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2. H. J. Greenberg, " The Principle of Limiting Stress for Structures." Paper presented to Second Symposium on Plasticity, Brown University (April 1949).
3. M. R. Horne, " Fundamental Propositions in the Plastic Theory of Structures." J. Instn Civ. Engrs, vol. 34, p. 174 (April 1950).
4. J. F. Baker, " The Design of Steel Frames." *Structural Engineer*, vol. 27, p. 397 (October 1949).
5. H. J. Greenberg and W. Prager, " Limit Design of Beams and Frames." Proc. Amer. Soc. Civ. Engrs, vol. 77. Separate No. 59 (Feb. 1951).
6. B. G. Neal and P. S. Symonds. " The Calculation of Collapse Loads for Framed Structures." J. Instn Civ. Engrs, vol. 35, p. 21 (November 1950).
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The Paper is accompanied by five sheets of diagrams from which the Figures in the text have been prepared.

Discussion

The Authors, introducing the Paper, assumed that the audience were all interested in plastic methods and had read the Paper presented by Professor J. F. Baker in 1949.[1] In that Paper, Professor Baker had pointed out that plastic methods of design were more rational than elastic methods and were in consequence bound to lead to economies of material. Plastic methods of design had to be based on a calculation of the plastic collapse load, that was to say, the load at which the deflexion of the structure could continue to grow while the load remained constant.

The concept of plastic collapse arose in the following way. If first the bending of a simple steel beam was considered, it would be found that if the bending moment tended to a certain limiting value (called the fully plastic moment) the curvature became extremely large. If the curvature became indefinitely large, it would be possible for a finite change of slope to occur in a very short length of the beam. In that case it could be said that a hinge existed at the particular cross-section concerned, and it would be observed that that hinge could rotate only when the bending moment was equal to the fully plastic moment.

It was obvious that infinite curvatures could not occur. On the other hand, very large curvatures did occur locally in practice, and it was found that the concept of a plastic hinge which could rotate only when the fully plastic moment was achieved was a very good approximation, and that by the aid of that concept it was possible to describe the behaviour of steel-framed structures very closely.

Passing on to the behaviour of a complete steel frame upon which the loads were steadily increased, it would be seen that hinges would form successively at different cross-sections of the structure in turn, until eventually a sufficient number of hinges had formed to enable the structure to deform as a mechanism. When that state was reached, deformations could continue to occur under a constant load, and the stanchion would then be in a state of plastic collapse.

The proposal was to design structures so that the working loads on the structure, when multiplied by some chosen load factor, were less than or equal to the calculated plastic collapse loads. It was therefore possible to formulate the plastic design problem as followed. For a given frame and loading it was required to find the correct collapse mechanism and also the corresponding collapse load.

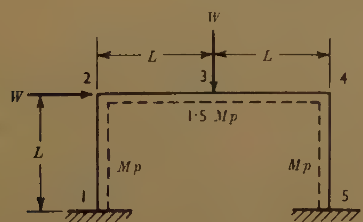
In the Paper it was pointed out that, for a given frame and loading, all possible mechanisms could be built up from a certain number of independent components. The Paper showed how to combine those component mechanisms in order to arrive at the correct collapse mechanism. It was

[1] J. F. Baker, "A Review of Recent Investigations into the Behaviour of Steel Frames in the Plastic Range." J. Instn Civ. Engrs, vol. 31, p. 188 (Jan. 1949).

essential to take the correct number of independent mechanism components, because otherwise the one which was missed out might be an essential one. On p. 69 it was stated that the correct number of independent mechanisms was precisely equal to the number of independent equations of equilibrium of the structure. The Authors emphasized that that was an essential point, and explained it by means of the following very simple example (which would also serve to indicate the technique which was applied to the far more complicated example given in the Paper).

Fig. 12 showed a simple rectangular portal frame with a horizontal load W and a vertical load W. The height was L and the span of the beam $2L$. The fully plastic moment of the two vertical members was M_p and the fully plastic moment of the beam was $1{\cdot}5\ M_p$. In applying the suggested technique the Paper made no mention of equilibrium equations, but they were introduced here in order to explain the technique of

Fig. 12



Equilibrium equations:

$$WL = M_2 - M_1 + M_5 - M_4 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$WL = 2M_3 - M_2 - M_4 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the Paper. To do so, it was therefore necessary to set up a sign convention for the bending moments at the five sections 1, 2, 3, 4, and 5. A positive bending movement caused tension in the flange of the member adjacent to the dotted line ; in other words, a positive bending moment caused sagging of the frame inwards. With that sign convention, it was possible to set up two equations of equilibrium. How was it possible to be sure that there were only two equations ? Five bending moments, at the sections 1, 2, 3, 4, and 5, were necessary to specify the bending-moment distribution for the entire frame because along the member 1–2, for example, the shear force was constant and the bending moment varied linearly, and therefore the bending moments at 1 and 2 would specify the bending-moment distribution in the member 1–2 ; similar reasoning applied to the other four segments, so that there were five unknown bending moments. The structure had three redundancies, because if a cut were made at any cross-section, as for instance at cross-section 1, and at that section the bending moment, thrust, and shear force were specified, the entire frame was statically

determinate. Since there were five known bending moments and three redundancies, there had to be two equations of equilibrium connecting the five bending moments.

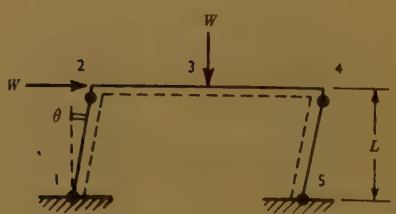
It was fairly easy to set up those equations. The horizontal load W must be carried by the shear forces in the member 1–2 and in the member 4–5. The shear force in the member 1–2 was equal to the rate of change of bending moment, which was $\frac{M_2 - M_1}{L}$, and the shear force in the member 4–5 was $\frac{M_5 - M_4}{L}$, so that the first equation was :

$$W = \frac{M_2 - M_1}{L} + \frac{M_5 - M_4}{L} \quad . \; . \; . \; . \; . \; . \quad (5)$$

Vhere was a similar equation of equilibrium for the beam. The vertical load W was carried by the shear force in member 2–3 and member 3–4, so that the second equation was :

$$W = \frac{M_3 - M_2}{L} + \frac{M_3 - M_4}{L} \quad . \; . \; . \; . \; . \; . \quad (6)$$

Fig. 13



Virtual work: $WL\theta = 4M_p\theta$, $W = \frac{4M_p}{L}$.

The corresponding equilibrium equation is :

$$WL = M_2 - M_1 + M_5 - M_4 \quad . \; . \; . \; . \; . \; . \; . \quad (5)$$

$$M_2 = M_p;\; M_1 = -M_p;\; M_5 = M_p;\; \text{and } M_4 = -M_p.$$

Substituting in equation (6) gives $M_3 = 2M_p$.

The next step was to consider one possible mechanism of collapse, the side-sway mechanism with hinges at sections 1, 2, 4, and 5 (*Fig. 13*). The hinges at 2 and 4 were put in the stanchions rather than the beam, because the beam was stronger than the stanchions. The Principle of Virtual Work could be applied to that mechanism, in the form that the work done by the side load W in the small motion of that mechanism was equal to the work absorbed in the four plastic hinges during that same small motion. If the inclination of the stanchions to the vertical was θ, as

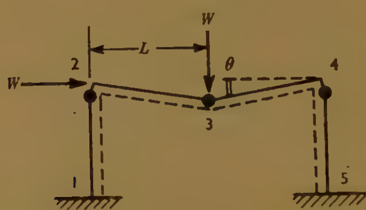
shown, each of the four hinges would rotate through an angle θ, but the plastic work was always positive, and the fully plastic moment at each section was M_p, so that the work absorbed was just $4M_p\theta$. The work done by the side load was $W \times$ the distance $L\theta$ through which it moved, so that $WL\theta = 4M_p\theta$ and $W = \frac{4M_p}{L}$.

That led to the crucial point, which was that that mechanism corresponded precisely to the breaking-down of equilibrium equation (5). If that equation was examined it would be seen that for that mechanism each bending moment took on its fully plastic value, and further, the sign of each bending moment was such as to maximize the right-hand side of equation (5). M_2, it would be seen, was $+ M_p$, because the hinge was opening out when viewed from inside; there was tension on the right-hand side of the stanchion. M_1, on the other hand, was $- M_p$. M_5 was $+ M_p$ and M_4 was $- M_p$. From the point of view, therefore, of that equation alone, W could not possibly exceed $\frac{4M_p}{L}$; equilibrium must be maintained, and the moments could not exceed the magnitude M_p. The largest possible value of W, therefore, from the point of view of equation (5) was $\frac{4M_p}{L}$. It would appear, therefore, that the equilibrium equation (5) corresponded to the mechanism of *Fig. 13*. Moreover, the Virtual Work result specified an upper limit on the value of W. It was known that W could not possibly exceed the value $\frac{4M_p}{L}$. By substituting the moments in equation (6) it would be found that the value of M_3 was $2M_p$. That bending moment would have to be $2M_p$ from the equilibrium point of view if there were those four plastic hinges. That was impossible, because the fully plastic moment at 3 was only $1{\cdot}5\ M_p$, so that it was obvious that the wrong mechanism had been chosen.

The next step, therefore, was to examine another possible mechanism failure of the beam by itself (*Fig. 14*). Hinges were shown once again in the stanchions rather than the beam, and there was a central hinge. If the angle between 3–4 and the horizontal was θ, the central hinge rotated through 2θ, and the other hinges through θ, so that the Virtual Work absorbed in each of the hinges 2 and 4 was $M_p \times \theta$, and in hinge 3 was $1{\cdot}5M_p \times 2\theta$. The total was $5M_p\theta$, and the work done by vertical load was $WL\theta$. For that mechanism W would have to be $\frac{5M_p}{L}$. The corresponding equilibrium equation in that case was equation (6). Each of the moments at the sections 2, 3, and 4 appeared in that equation, with their fully plastic moments in the mechanism, and if the signs were examined it would again be seen that the signs were such as to maximize the right-hand side of the equation. M_3 was $+ 1{\cdot}5M_p$; M_2 was $- M_p$; and M_4 was $- M_p$.

Equilibrium equation (6), therefore, corresponded to that mechanism in the sense that the mechanism expressed the breaking down of equation (6). From the point of view of equation (6), W could not possibly exceed $\frac{5M_p}{L}$. Substituting the moments in equation (5), it would be found that $M_5 - M_1$

Fig. 14



Virtual work : $WL\theta = 5M_p\theta$;

Whence $W = \frac{5M_p}{L}$.

The corresponding equilibrium equation is :

$$WL = 2M_3 - M_2 - M_4 \quad \ldots\ldots\ldots\ldots \quad (6)$$

$M_3 = 1{\cdot}5\, M_p$; $M_2 = -M_p$; and $M_4 = -M_p$.

Substituting in equation (5) gives $M_5 - M_1 = 5M_p$.

was $5M_p$, so that it was not possible to determine M_5 and M_1 separately. Since the fully plastic moments at 5 and 1 were M_p, the largest possible value of $M_5 - M_1$ was $2M_p$, so again the correct solution had not been obtained.

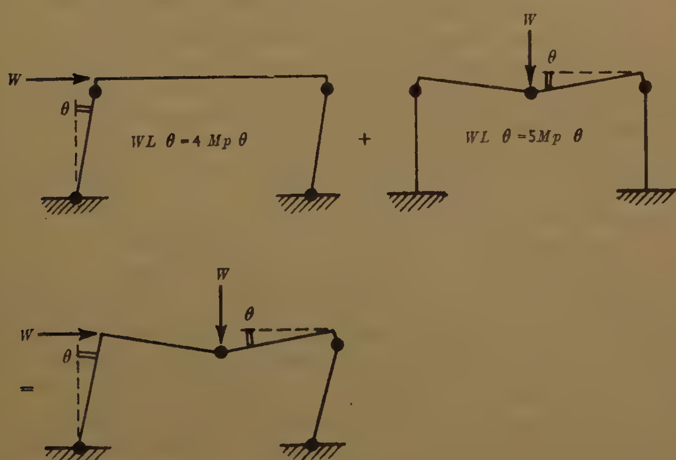
Examining those two mechanisms again, it would be seen that if the displacements and hinge rotations of the beam mechanism were superposed on those of the side-sway mechanism the result shown in *Figs 15* was obtained. The central load W now moved through a distance $L\theta$, and so did the horizontal load. The hinge at section 2 was cancelled, the rotations being of opposite sense. All the other hinge rotations added up with the result shown. It would be possible, of course, to apply Virtual Work directly to this third mechanism. However, using the two results of $WL\theta = 4M_p\theta$ in the one case and $WL\theta = 5M_p\theta$ in the other, the Virtual Work equation for the third mechanism could be deduced immediately. In that mechanism the movements of the loads were obtained by summing the movements in the first two mechanisms, so that the Virtual Work done by the loads added up to $2WL\theta$. The Virtual Work absorbed in all the hinges could also be summed. The hinge at section 2 had been cancelled out, as explained earlier. There was no Virtual Work absorbed at that section in the resulting mechanism, so that after performing the addition it was necessary to subtract $2M_p\theta$. The Virtual Work calculation for the combined mechanism could therefore be set out as follows. :—

$$\begin{aligned} WL\theta &= 4M_p\theta \\ WL\theta &= 5M_p\theta \\ \hline 2WL\theta &= 9M_p\theta - 2M_p\theta = 7M_p\theta \end{aligned}$$

where $$W = \frac{3{\cdot}5M_p}{L}$$

Since that mechanism was compounded of the other two, it would be expected that its corresponding equilibrium equation could be obtained

Figs 15



by compounding the two equilibrium equations already obtained. That was in fact so. By adding equations (5) and (6), M_2 cancelled out leaving :—

$$2WL = 2M_3 - M_1 + M_5 - 2M_4. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

If the breakdown condition for equilibrium equation (7) were examined, it would be seen that, for breakdown, M_3 would have its largest value, $1{\cdot}5M_p$; M_1 would have its smallest value, $-M_p$; M_5 would be $+M_p$; and M_4 would be $-M_p$. Those were the fully plastic moments appearing in the combined mechanism shown in *Figs 15*, and the corresponding value of W was $\dfrac{3{\cdot}5M_p}{L}$. This mechanism was compounded, therefore, of the two independent components, simply because there were only two independent equations of equilibrium. Every mechanism corresponded to the breakdown of an equilibrium equation and therefore that third mechanism must correspond to the breakdown of an equilibrium equation which was obtained from the first two independent ones. There was a precise

correspondence between a mechanism and an equilibrium equation, and therefore in a general problem in order to count the number of independent mechanisms it was merely necessary to count the number of independent equations of equilibrium. It should be observed that, substituting back in equations (5) or (6), the value of M_2 was $\frac{1}{2}M_p$, which was within the allowable limits of $\pm M_p$, and therefore that was the correct solution for the problem.

As he had said, every mechanism corresponded to the breakdown of an equilibrium equation, and so the correct mechanism must be the one to which there corresponded the smallest possible value of W. It had been seen that equilibrium equation (5) broke down when W was $\frac{4M_p}{L}$, and equation (6) when W was $\frac{5M_p}{L}$, but at that value of W the first equation would already have broken down, and the combination of the equations would have broken down at $\frac{3 \cdot 5M_p}{L}$. Thus the correct value of W was the smallest which could be found, and that was the guiding factor in combining mechanisms.

Professor J. F. Baker observed that when designing a structure it was necessary to define the mode of collapse and, up to the present, in choosing the economical mode of collapse, reliance had had to be placed on intuition or on trial-and-error methods. For the single-bay single-storey rectangular portal not much intuition was necessary and no trial and error was required, and even when dealing with the single-storey multi-bay frame, even with pitched roof and distributed loads, trial-and-error methods were not intolerable and it had been possible to get on quite well. As soon, however, as anything more complex was attempted, such as a multi-storey frame, the number of possible modes was so great that trial and error became intolerably arduous.

The new method of analysis—and it should be noted how careful the Authors had been not to claim anything more than that for it—which was described in the Paper would be of very great help in developing a general method of designing economical structures, which was the research engineer's great aim.

He thought it was unfortunate in some ways that the Authors had chosen for their illustration a building frame, because inevitably they would be faced with questions of stability and the behaviour of columns, questions which it was not yet possible to answer in a short Paper. It had been due to his own stubbornness and past history that so much of the Cambridge's team's efforts had been directed to building structure. That form of structure was inherently difficult, and had been made more so by the fact that it had for so many generations been coddled in building regulations. He had said more than once, at Meetings of the Institution, that no method

of design for a building structure was considered successful unless it could be written on one side of a postcard.

Fortunately for engineers who were interested in those matters, however, there were structures other than building structures; but it would be intolerable if even for those it was necessary to wait to benefit from any new advance until that advance had been so simplified that it could be applied by anyone. That was no longer the case. The University of Cambridge had now instituted a post-graduate course which could be attended by any engineer with reasonable academic qualifications and an adequate practical experience, where he could be instructed in anything which had been done at Cambridge in the way of plastic design and allied subjects. It was encouraging that every student on the first course, now being conducted, had been sent by his firm and was being supported by his firm. Those men would, after a few months, be in a position to go back to industry and apply what they had learned, and so cut down very appreciably the time between the development of a new approach and its application, a time which normally had been such a handicap to the industries of Great Britain. In his opinion that type of post-graduate course, which was being increasingly adopted by British universities, was a great advance in the sphere of higher technological education which was so much discussed to-day.

Mr R. A. Foulkes congratulated the Authors on a Paper which, he said, represented a very real and important step forward in the plastic method of design. The physical picture which was presented of the way in which the structure collapsed and the way the collapsed mechanism was built up, together with the absence of any need for sign conventions, would appeal to engineers.

Taking *Fig. 1* of the Paper, he had assumed a storey-height of 10 feet and a span for the beams of 20 feet, and he had taken the frames to be at 12-foot-6-inch centres. He had taken W to correspond to a superficial load of 75 lb. per square foot, so that the actual value of W was rather more than 8 tons. It was then possible to take the value of M_p from p. 68 of the Paper and to fit sections to the beams. Those numbered 1 would be 10-inch-by-5-inch, those numbered 2 would be 15-inch-by-5-inch, whilst those numbered 3 would be 18-inch-by-6-inch I-beams. By that time, however, there was a considerable total load on the frame, approximately 218 tons, and about half of that would come down the centre stanchion. The effect of that load on the 18-inch-by-6-inch joists used for HK was to reduce the plastic moment by approximately 30 per cent, so that the initial value of the ratio 3 had now been reduced to 2·1. It was not possible to get the exact value until the collapse mechanism was known, because the actual thrusts would depend on the plastic hinge positions.

In a much simpler frame which he had analysed some time ago the load factor at collapse, neglecting thrust, had been 2·5, whilst taking into account the actual thrust at each plastic hinge position it had been only

2·3. He felt, therefore, that the effect of thrust could not be ignored. It would appear to have even more influence on the load factor than the precise positioning of the plastic hinges.

There would probably be general agreement that the plastic analysis presented in the Paper was a far shorter method for analysing a frame of the kind in question than anything which could be done on the elastic side. It was also reassuring to know that actual frames, when loaded up to collapse, did behave in the way shown; their plastic hinges came in the right places and the agreement with the calculated load factor was very good.

Dr Jacques Heyman observed that the method presented by the Authors for the determination both of the collapse load, and of the mode of plastic deformation of a framed structure, had particular advantages when the action of several independently applied loads was considered. In orthodox elastic design based on a limiting stress at the critical sections of the frame, the stress produced by any one load could be determined without reference to the other loads; that was to say, the principle of superposition could be applied. For the check analysis of an elastically designed frame, it was sufficient for the designer to add up in the worst possible way the stresses at any one section produced by the various loads, and to ensure that the maximum computed stress was less than the allowable stress at that section. In plastic design, however, the principle of superposition no longer applied; the presence, absence, or reversal of any one of the loads might alter radically the mode of collapse of the frame and the corresponding load factor. In general, each combination of loads must be considered on its merits.

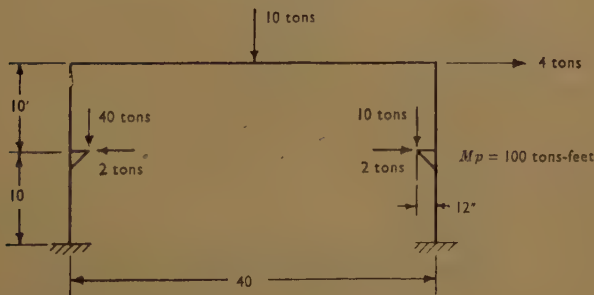
Figs 16 showed a simple single-bay rectangular portal frame which he had taken as being subjected to a dead and snow load, represented by the concentrated load of 10 tons acting at the centre of the beam, together with a wind load of 4 tons acting horizontally at the level of the beam. Those were ideal loads and simply for the purpose of analysis, and would be taken as fixed for the purposes of the example. In addition, the frame housed a travelling crane. If the crane crab was to the left, the load on the left-hand stanchion bracket would be 40 tons, and the load on the right-hand stanchion bracket 10 tons; if the crane crab was to the right, those loads would be interchanged. In addition, the crane could surge, producing a force of 2 tons, which might be absent or might act on either of the stanchions, but not on both together.

It was a difficult matter, on the face of it, to decide which was the worst combination of loads. The most critical condition for the frame might occur when the surge force of 2 tons acted in the same direction as the wind force of 4 tons, but it was not easy to see which would be the worst position for the crab, whether to the left or to the right. However, the method of combining independent collapse mechanisms led very quickly to the determination of the critical loading condition.

He proposed to replace those variable forces by symbols, and *Fig. 16* (*b*) showed the frame subjected to the loads of 10 and 4 tons, to forces R_1 and R_2, and to moments C_1 and C_2 at the centres of the two stanchions. R_1 and R_2 were either both zero or one of them had the value of 2 tons and the other zero. The point of application of the vertical crane loads had been taken as 1 foot distant from the centre-lines of

Figs 16

(*a*)



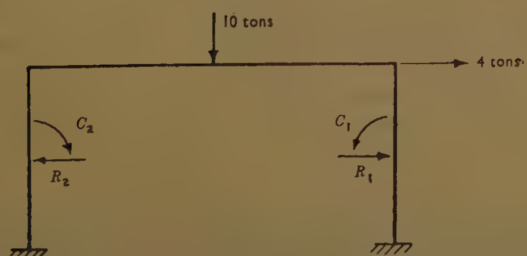
Crane crab at either end.

Crane surge either absent or causing horizontal load of 2 tons on either stanchion

$$\left.\begin{aligned} R_1 &= R_2 = 0 \\ \text{or } R_1 &= 2,\ R_2 = 0 \\ \text{or } R_1 &= 0,\ R_2 = 2 \end{aligned}\right\}$$

$$\left.\begin{aligned} C_1 &= 10,\ C_2 = 40 \\ \text{or } C_1 &= 40,\ C_2 = 10 \end{aligned}\right\}$$

(*b*)

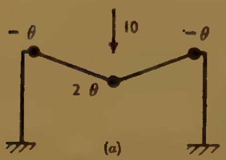


the stanchions, so that the load of 40 tons, for example, might be replaced by a moment of 40 tons-feet, together with an axial thrust in the lower half of the stanchion. For present purposes, the effect of those axial thrusts would be ignored; in fact, the thrust in the more heavily loaded stanchion would reduce the fully plastic moment by about 20 per cent in the example given, and allowance would certainly have to be made for that in a practical design. The moments C_1 and C_2 had values of 40 and

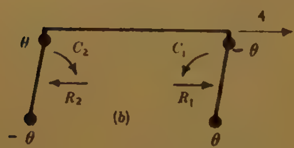
10 tons-feet respectively if the crab was to the right, and those values were interchanged if the crab was to the left.

There were seven critical sections in the frame where plastic hinges might be developed: the centre and ends of the beam, and the centres and feet of the two stanchions. Since the frame had three redundancies, four independent collapse mechanisms were required; those might be taken as the mechanisms (*a*), (*b*), (*c*), and (*d*) shown in *Figs 17.* Assuming

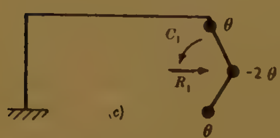
Figs 17



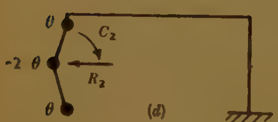
$$q = \frac{4(100)}{200}$$



$$q = \frac{4(100)}{80 + 10(R_1 \quad R_2) + C_2 - C_1}$$



$$q = \frac{4(100)}{10R_1 + C_1}$$



$$q = \frac{4(100)}{10R_2 + C_2}$$

Worst cases

	a	b	c	d
R_1	—	2	2	0
R_2	—	0	0	2
C_1	—	10	40	10
C_2	—	40	10	40
q	2·00	3·08	6·67	6·67

INDEPENDENT COLLAPSE MECHANISMS

the frame to be made of a uniform section with a fully plastic moment of 100 tons-feet, the factors q by which the loads must be multiplied in order to produce collapse by each of those mechanisms might be calculated. Each expression for q was the ratio of the work done in the hinges to the work done by the loads.

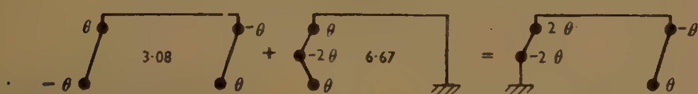
For the first mechanism, (*a*), pure beam failure, a value of 2 was obtained for the load factor. Mechanisms (*b*) pure side-sway, (*c*) the right-hand stanchion failing as a beam, and (*d*) the left-hand stanchion failing as a beam, however, gave values of q which contained in the denominator expressions involving R_1, R_2, C_1, and C_2. For mechanism (*c*), for example,

the denominator was $(10R_1 + C_1)$, and the lowest value of the load factor would obviously be attained when both R_1 and C_1 had their maximum values of 2 and 40 respectively.

The Table shown in *Figs 17* gave for each mechanism the values of the forces and moments to produce the lowest load factor. It would be seen that different loading cases arose for each of the independent mechanisms. The Table corresponded very roughly to the sort of loading conditions that would be considered in elastic design, the stresses in the left-hand stanchion being computed when the crane crab was to the left, and those in the right-hand stanchion when the crab was to the right. Orthodox elastic design was to that extent somewhat irrational, in that different portions of a

Figs 18

(i)



(b) (d) (e)

$$q = \frac{4(100) + 4(100) - 2(100)}{80 + 10R_1 + 2C_2 - C_1}$$

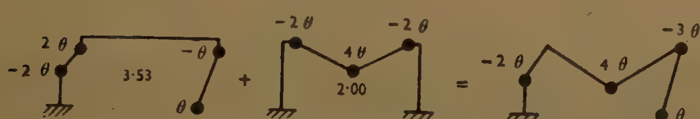
Worst case for $R_1 = 2$ $(R_2 = 0)$

$C_2 = 40$

$C_1 = 10$

$q = 3{\cdot}53$

(ii)



(e) 2 (a)

$$q = \frac{8(100) + 6(100) - 4(100)}{480 + 10R_1 + 2C_2 - C_1}$$

Worst case : $q = 1{\cdot}75.$

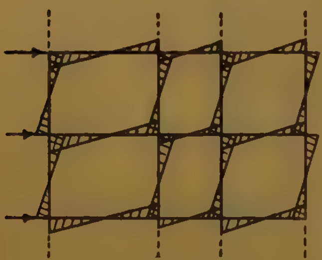
structure were designed under different loads. For a rigid-framed structure, on the other hand, there would certainly be one combination of loads which, when multiplied by the design load factor, caused the structure to collapse as a whole, and that combination could be found most easily by combining the independent collapse mechanisms.

Figs 18 showed the effect of combining mechanisms (*b*) and (*d*) to produce a new mechanism, (*e*). The value q of the load factor for mechanism (*e*) was written as the sum of the numerators divided by the sum of the denominators of the load factors for mechanisms (*b*) and (*d*). In addition, since the rotation at the left-hand stanchion foot for mechanism

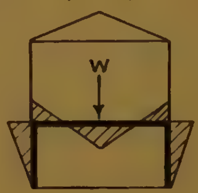
(*b*) was equal and opposite to that for (*d*), twice the fully plastic moment (200) could be subtracted from the numerator, and if R_1, C_2, and C_1 were given their most unfavourable values, a value of $q = 3{\cdot}53$ resulted. That value was higher than the value for (*b*) alone; howevei, if the new mechanism (*e*) was combined with the mechanism (*a*), as shown in *Figs 18* (ii), the least possible value of $q = 1{\cdot}75$ was obtained, and no further combination of mechanisms produced a lower value.

If the plastic analysis of a frame was undertaken in that way, using symbols for loads that might act in either direction or have variable values, it was very easy to see, at each stage in the calculations, which were the critical loading conditions, and whether the combination with another collapse mechanism in an attempt to lower the load factor altered those critical loads.

Figs 19


(*a*)


(*b*)



(*c*)

Professor A. L. L. Baker said that the new method of locating plastic hinges, described in the Paper, was another step forward in the development of plastic theory.

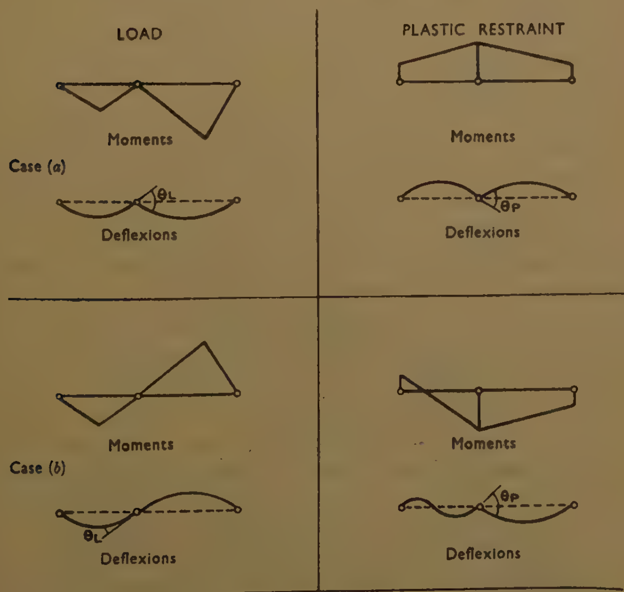
It might be of interest to consider that problem in relation to reinforced-concrete frameworks, and to suggest some points which might also apply to structural steelwork. Probably the best approach to the problem was by a procedure of design and adjustment, rather than by direct analysis of an assumed framework.

The designer needed to have a good knowledge and experience of elastic and plastic theory so that he could assume approximate dimensions of a framework which, prior to collapse, would develop a distribution of bending moments as close as possible to those shown in *Figs 19* (*a*) and (*b*), that was, with mid-span and support moments equal in the beams and points of contraflexure at mid-height in the columns. It might not always be advantageous to equalize before collapse the support and mid-span moments, since other requirements such as shear and compression strength, relative cost of centering steel and concrete required consideration.

Starting from the top of the framework the designer would work downwards, assuming the positions of sufficient plastic hinges to make the framework statically determinate. Each hinge and the adjacent members had then to be considered in turn. The strength and stiffness of members might be varied locally by adjusting the reinforcement until the following conditions were satisfied :—

(1) The sign of the angle of discontinuity of each hinge had to be such that it was opposed to the direction of the plastic moment of the hinge, and its value not in excess of the value at which the concrete would crush.
(2) The stresses between the assumed plastic hinges had to be within the elastic range.
(3) the strains under working load had to be small enough to prevent excessively wide cracks forming or concrete spalling.

Figs 20



MOMENTS AND DEFLEXIONS OF STRAIGHTENED MEMBERS

Referring to *Figs 20*, cases (*a*) and (*b*) it would be seen that if $\theta_L > \theta_P$ and between the hinges the stresses were within the elastic range, the positions had been correctly chosen. It was necessary to apply both those checks since it was possible for θ_L to be $> \theta_P$ and the elastic limit to be exceeded between the assumed hinges, particularly when there was a local weak section, or an adjacent weak or very flexible member, or a

point of contraflexure near the assumed hinge point due to reversed loading. It was necessary to check that θ_L was greater than θ_P, because the assumed plastic hinge moments might not develop until plastic hinges had formed at some other point due to the resultant bending moment at that point being greater than the assumed amount. In such a case the hinge positions assumed might be ill-chosen, so that they would not form until the structure had become a mechanism, at which stage some of the hinges might have taken up excessive rotation. In a case such as that shown in *Fig. 19* (*c*), in which the lower portal was very much stiffer than the upper, it was clear that excessive rotation could easily occur in the hinges in the lower portal before the whole structure became a mechanism or even while the upper portal structure was still elastic.

The procedure of trial and adjustment was greatly assisted by using the δ_{ik} notation and general elastic equations. The usual unknown x-values were substituted by $\bar{x}$, the known plastic hinge moment values, and the sum of the deformations at any hinge was equated to an unknown angle of rotation θ, whose value and sign could then be found by simple arithmetic. In a case such as *Fig. 19* (*c*), if rotations in the stiffer part were likely to be excessive before even hinges form in the more flexible part, unknown moments of resistance in the very flexible part of the frame might be assumed in the usual way, to make the structure statically determinate, and the sum of the deformations at those hinges equated to zero. The solution of a few simultaneous equations was then involved. Did the Authors consider that, in welded steel frames, designing for a collapse load with the frame acting as a mechanism would be satisfactory, in view of the high strains which might occur under working load ?

As the values and signs of the angles of discontinuity were checked, local adjustments to the reinforcement and binding could be made, and perhaps occasionally a hinge position altered until all the members and hinges satisfied the specified conditions. A load which was sufficient to cause enough hinges to be formed to make the structure statically indeterminate could then be found, and assumed to be the collapse load. The stresses under working load could be found approximately by using direct moment distribution, or a method of moment distribution which applied to the frame bending moments which would close the angles of discontinuity.

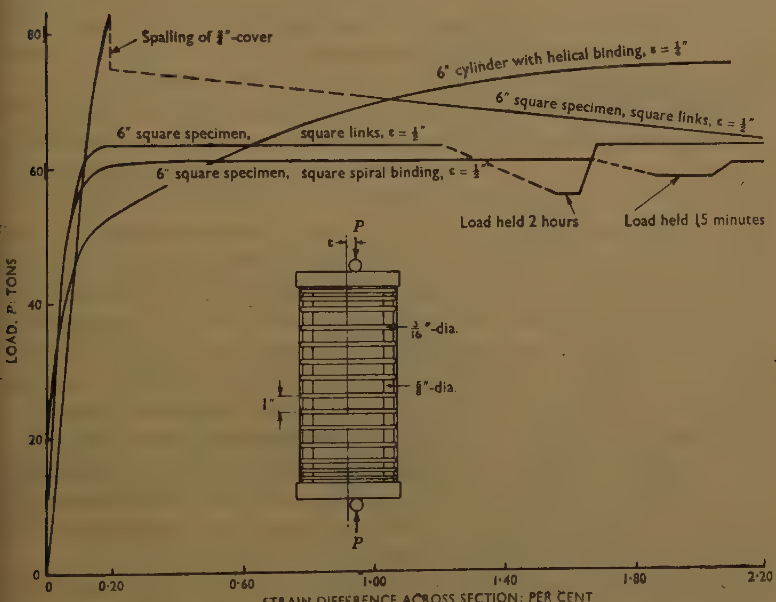
Fig. 21 showed the load/strain relations of some eccentrically loaded wire-bound prisms, recently tested at Imperial College, London. They indicated that it was probable that further research would provide the necessary fundamental data which would enable suitable binding reinforcement to be used to obtain adequate rotation of plastic hinges, particularly in the columns of frameworks.

The possibility of repeated partial loading causing failure at load values smaller than those required for a single application of total load required some consideration. That matter could probably best be dealt

with by assuming that the chances of repeated overload occurring on certain parts of the structure in a particular sequence was much more remote than the chance of total collapse load occurring once.

There had been much criticism of the plastic theory, but there were many cases in which there was no alternative way of calculating the factor of safety. Moreover, elastic theory calculations, so often laboriously precise, were frequently based on assumptions which were not true ; for example—influential factors were often ignored, such as relative settlement of supports, slip of the joints in steel work, shrinkage, creep, and

Fig. 21



non-uniform *EI* values in concrete. There were two possible general approaches to the design of frameworks, one by using the elastic theory and making adjustments to allow for conditions which might occur before collapse, and the other to calculate the collapse load by plastic theory and check approximately the stresses under working load. Designers had to select, in any given case, the best approach to use, and both working and ultimate conditions had to be considered. There was much research and clarification yet to be done in regard to the plastic theory, but research already carried out at Cambridge and elsewhere had indicated the future practical value of the theory which, it was anticipated, would be brought more and more into use. To become a competent designer of statically

indeterminate frameworks, an engineer would need to be a master of both theories and to know how, why, and when to apply them.

Mr N. S. Boulton said that to apply the Authors' method the relative values of the fully plastic moments must be known. In an attempt to obtain, by a simple procedure, a frame approximating to minimum weight for a given collapse load, the fully plastic moments shown in *Fig. 1* could be modified as followed :—the beam values were first chosen so as to give the same collapse load, and then, taking M_p as unity for the beam AB it was found that $W = 4$ for local collapse of each beam. The fully plastic moments for the stanchions were next adjusted so that the elementary sway mechanisms in *Figs 4, 5,* and *6* had the same collapse load. Solving the resulting frame by the method described in the Paper gave $W = 2{\cdot}73$ at the stage before the positions of the beam hinges were adjusted. The combined collapse mechanism was the same as that shown in *Fig. 9,* except that plastic hinges occurred in AB and BC.

Extending the equalizing procedure to combined mechanisms, the fully plastic moments of beams ABC and DEF—the two top ones—could then be modified so as to make W the same for sway about stanchion hinges at D, E, and F, and alternatively at G, H, and I, as for the whole frame. These last modifications left the average fully plastic moment, say M_p, for the whole frame unchanged. It was found that the ratio $W/\overline{M}_p$ was 12 per cent greater than for the frame shown in *Fig. 1.* That meant that if the weights of the members were proportional to their fully plastic moments, the simple procedure outlined reduced the weight of frame required by 12 per cent. For rolled sections in which the weight was not proportional to the section modulus that reduction might be 8 per cent.

The procedure described, whilst unsubstantiated by strict reasoning, seemed a plausible way in which to improve the collapse-load/weight ratio. Did the Authors know of a more rational procedure of that kind which would be preferable in practice to Dr Heyman's method, involving as it did the solution of a large number of inequalities ?

Mr J. D. Foulkes said that there were two points in the Paper which deserved more emphasis than the Authors had been able to give them in the limited space available. The first was the statement on p. 60 that "To shorten the numerical work, each storey is taken to be of unit height, and each beam has a span of 2 units." It was worth noting that for rectangular frames it was always possible to reduce a frame to just such a 2 × 1 frame by the simple procedure of shortening members, or lengthening them, and simultaneously altering the loads which bore on the members inversely as their lengths were altered; for, since a bending moment was simply the product of a force and a length, it would clearly not be altered if forces and lengths were altered inversely to one another. That trick, in the case of pitched-roof portals and one or two other frames, unfortunately altered the geometrical angles of the frame, and in consequence was not of such great value as in rectangular frames. The trick

not only shortened the arithmetical labour in the solution but familiarized the designer with the behaviour of a single model, and rapidly built up a capital of experience upon which he could draw in analysing rectangular frames.

The second point concerned the order in which local collapse mechanisms were superimposed. It seemed at first sight that all that had to be done was to examine the load factors given by the individual or local collapse mechanisms, and then start with the one which gave the lowest and add in other local collapse mechanisms so as continually to reduce the load factor. Unfortunately, however, it frequently happened that a local collapse mechanism which could be added in with advantage in the early stages of calculation had later to be taken out if the correct result was to be reached. It would be seen, therefore, that it was necessary to scrutinize continually the contributions of individual local collapse mechanisms to the overall load factor in order to arrive at the correct result. That became increasingly tedious and increasingly important when the frame was a minimum-weight-design frame or a closely related design, for it could be shown that a minimum-weight-design frame of n members had n alternative mechanisms of collapse. It would be appreciated, therefore, that in such a frame or a closely related frame there would be very many local collapse mechanisms with slightly different load factors, and great care was necessary in order to determine just which local collapse mechanisms must be superimposed in order to give the correct result.

Dr M. R. Horne agreed with Professor J. F. Baker that it was perhaps unfortunate that the Authors had chosen a multi-storey building frame to illustrate their method of analysis; a casual reader of the Paper might imagine that here was a design method for multi-storey buildings. No such claim had been made by the Authors and, in order to appreciate the limitations of their analysis, it was important to realize exactly on what assumptions their method of calculating collapse loads was based.

It was assumed that if any structure were taken such as, for example, a multi-storey building frame, and equations of equilibrium for loads and moments set down, then those equations would not need to be modified right up to the point at which collapse occurred. If, however, there were a considerable amount of sway in the structure, obviously those equations of equilibrium would be invalidated, probably before sufficient plastic hinges had developed to form a mechanism in the way suggested in the Paper. Similarly, conditions of local instability causing a stanchion to bow, or the compression flange in a slender beam to buckle, could be interpreted as upsetting the equilibrium within those members. In all cases where such instability occurred the simple approach of finding collapse mechanisms with sufficient plastic hinges for collapse did not give the true collapse load. It was hoped by those who were working on the plastic theory that it would soon be possible to lay down the conditions under which simple "plastic hinge" methods of analysis would give a

good estimate of the collapse load and, if not, what special allowances should be made.

The Authors had indicated that there were three essential conditions which had to be satisfied by a bending-moment distribution deduced for a given structure, such that it should represent the conditions at collapse. The three conditions which had to be satisfied were :—(1) the equilibrium conditions, (2) the condition that the full plastic moment was nowhere exceeded, and (3) the condition that there were sufficient hinges for collapse to occur. In the Author's method, emphasis was placed upon (1) and (3). The correct mode of collapse was determined by finding the mechanism which gave the minimum load, when it could be shown that condition (2) would be automatically satisfied.

The method presented by the Author owed its rapidity largely to the fact that the conditions of equilibrium could be satisfied very easily by the Virtual Work equation ; but the method, although it was very rapid, had a certain disadvantage in that at any intermediate stage before the final result was obtained, the estimate of the collapse load was on the high side. The correct mechanism was that which gave the lowest load and, for that reason, he thought that the word " advantageous," as used by the Authors, was unfortunate, and might convey the wrong impression. The Authors had really been looking for the most " disadvantageous " combination of mechanisms, not the most " advantageous."

Since, in the method presented by the Authors, the mechanisms were combined in such a way as to get the worst load factor, the collapse load was approached from above. There was, however, an alternative approach, concentrating upon the first two conditions listed above, namely to satisfy all the time the equations of equilibrium and the condition that the fully plastic moment must not be exceeded. If it were possible to find bending-moment distributions satisfying (1) and (2), and if they could then be systematically modified until a collapsed state was reached with the necessary numbers of plastic hinges, a method of analysis which approached the collapse load from below would have been derived, so that at any intermediate stage a safe estimate of the collapse load would be obtained.

Such a method was to follow the whole analysis through step by step elastically and watch for plastic hinges to form, but that would be a very long and laborious process. He had, however, been able to produce a method involving a " plastic " moment distribution process which was quite reasonably rapid. It was not quite so rapid as the method which the Authors had described, but it had the advantage that it did approach the collapse load from below, and he mentioned it because it was essentially complementary to the method presented by the Authors.

Mr R. M. Haythornthwaite observed that the essential basis of the Authors' approach seemed to be to try every possible mode of collapse of a framework and then to select the mode giving the lowest estimate of the collapse load as being the correct one. They had shown that that was not

necessarily a lengthy procedure, even for quite complicated frames. However, might not some of those modes be rejected in advance ? As the Authors had shown, there was little chance of being able to avoid computing each of the local collapse modes, because they occurred independently ; but it might be possible to reject certain combined modes of collapse on the basis of the behaviour of the elementary modes.

Assuming that two of these elementary modes were independent, in the sense that the operation of one of the modes did not cause the loads associated with the other mode of collapse to be displaced, then it could be shown that a mode obtained by combining those could occur at a lower load only if the independent modes of collapse had a hinge in common, and the two senses of rotation at that hinge were opposite in sign. Applying that result to the illustrative example quoted in the Paper, it would be seen at once that nothing could be gained by combining the local mode of collapse of BC, *Fig. 3*, with the sway collapse mode illustrated in *Fig. 8*. He submitted the following formal proof of that proposition :

Define two collapse modes as independent collapse modes when the load system associated with each undergoes no corresponding displacement when the other mode occurs. The theorem states that a mode of collapse obtained by combining two independent modes cannot be preferred to both of them unless the independent collapse modes have a plastic hinge in common at which the senses of rotation are opposed.

Proof: Suppose a mode of collapse associated with the displacement of a system of applied loads P_m' involves a set of rotations ϕ_i at i plastic hinges when the corresponding displacements of the loads P_m' are δ_m. By the Principle of Virtual Work :—

$$\Sigma P_m'\delta_m = \Sigma M_i\phi_i = \Sigma|M_i||\phi_i| \quad (i = 1, 2, \ldots r, r+1, \ldots r+s)$$

where M_i is the full plastic moment at the i^{th} hinge. The sign of M_i is chosen so that the product $M\phi_i$ is always positive. Suppose a second mode of collapse to be associated with a load system Q_n' ; then similarly :—

$$\Sigma Q_n'\delta_n = \Sigma M_i\psi_i = \Sigma|M_i||\psi_i| \quad (i = r+1, r+2 \ldots r+s, \ldots t)$$

Make the above modes independent. The loading is proportional, so let $Q_i = cP_i$ where c is a constant. Considering the same displacements δ_m and δ_n in the mode obtained by combining the above independent modes :—

$$\Sigma P_m''\delta_m + \Sigma cP_n''\delta_n = \Sigma M_i\theta_i = \Sigma|M_i||\theta_i| \quad (i = 1, 2, \ldots t). \quad . \quad . \quad (7)$$

In the independent modes, the rotations are proportional to the corresponding displacements, which are held constant ; hence the Principle of Superposition will apply to the rotations. Thus $\theta_i = \phi_i + \psi_i$. P_m'' can be less than P_m' only if $|\phi_i + \psi_i| < |\phi_i| + |\psi_i|$ for one or more of the hinges. This is so if ϕ_i and ψ_i are of opposite sense and if $\phi_i \neq 0$ and $\psi_i \neq 0$ for the hinge concerned.

Q.E.D.

NOTE :—The right-hand side of equation (7) will be smallest when one or more of the rotations θ_i is zero. Thus the smallest collapse load for some combined mode will occur when one or more of the plastic hinges present in the independent modes is eliminated.

Mr D. A. Howells said that there was a difficulty in the application to design of the theory of the behaviour of steel frames in the plastic range ; namely, that of specifying the loading system. The Authors had shown themselves to be aware of that when they had said, in an earlier

Paper,[1] ' A procedure which will probably be found suitable in many cases will be to base the design of the structure on the principles of plastic collapse, even though it is known that the loads will not remain in constant proportion." They did not seem to regard that as a serious difficulty, and on p. 61 of the present Paper they had said " The loads are all specified in terms of a single parameter *W*, and the problem is to determine the value of *W* at which plastic collapse would occur, ..."

For the designer, the implication of those statements was twofold, first that collapse loads could be formulated by extrapolation from working loads, and secondly that a satisfactory rule for extrapolation was to multiply the working loads by a single numerical factor. Both those propositions appeared doubtful, and evidence to support them from observations of the real world outside the laboratory would be welcome.

Whether a statistical investigation into cases of collapse or near collapse which had happened in the past would yield fruitful evidence of the loading systems which were their cause was an interesting speculation. The Tacoma Narrows bridge was a different type of structure from those now under discussion, but its collapse load was an interesting example of a loading system which could occur, and was very different from the loads which would be predicted by factoring the working loads. It was doubtful whether the designer could derive all the benefits of the plastic theory until the question of load factors had received more critical examination. It was necessary to bear in mind the words of Professor Pugsley[2]: " It is so easy to estimate what appears to be a reasonable working or extreme load and then find in the field that such a load is remote from reality. . . ."

The Chairman asked Dr Neal whether he could say what was meant by the word " rapid " in the title of the Paper. The Chairman had no idea of how long it would take to analyse the frame described, and it would interest all those present to know how long it had taken the Authors to solve that particular problem.

At the foot of p. 68 there began an " argument due to Greenberg and Prager " on something which seemed perfectly obvious. If any load system were applied to the structure shown in *Fig. 1*, and that load increased steadily and proportionately, the structure would collapse at a certain multiple of the original loads in the correct mode. Any other assumed mode of failure would clearly require a higher load.

Mr G. V. Kibblewhite thanked the Authors for having written a Paper which he could understand. A little over 20 years ago, he said, his Professor of Civil Engineering had said, in the course of a lecture, " Mathematics is only useful to the engineer when it is easy." He need hardly say

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[1] P. S. Symonds and B. G. Neal, " The Calculation of Failure Loads on Plane Frames under Arbitrary Loading Programmes." J. Instn Civ. Engrs, vol. 35, p. 41 (Nov. 1950). See p. 59.

[2] A. G. Pugsley, " Concepts of Safety in Structural Engineering." J. Instn Civ. Engrs, vol. 36, p. 5 (March 1951). See p. 28.

that those were the only words of the lecture which he now remembered, but he did remember that at the time he had been pleased to have that endorsement of a conclusion which he had already reached himself! He therefore appreciated the clear and simple way in which the subject had been presented.

Referring to a point raised by Professor A. L. L. Baker—a structure where there was both elastic and plastic deformation—Mr Kibblewhite said he was not clear whether it was safe to ignore the elastic deformation, as appeared to have been done in the Paper. He could imagine a frame where a number of elastic deformations in various parts of the structure could add up to induce excessive plastic deformation in another part of the structure.

Would the Authors say something about their model-results? That, after all, would be the proof.

It would be interesting to hear more about the publications of the British Constructional Steelwork Association. Their publication No. 3 had been clear and simple, and it contained the promise that design recommendations would be published which seemed likely to overlap the subject of the present Paper. He thought that both Papers had a common ancestry in Professor J. F. Baker's Paper (Reference 1, p. 71), but the Constructional Steelwork Association seemed to be working on rather different lines.

Mr D. H. Little remarked that he was not sure whether the previous speaker really understood the Paper or not, but all the other speakers appeared to have done so, and Mr Little was afraid that he did not. The Admiralty, however, over the past 2 years had done a certain amount of designing on plastic theory, and had adopted the principle of drawing up designs themselves and then getting Professor J. F. Baker's views on them. Mr Little had done his best to understand plastic theory, but he did find it a little difficult; however, those who had had more time than he to study it had assured him that it was quite simple. The big stumbling block to the average practising engineer was going to be, he thought, the need to think in terms of plasticity instead of elasticity.

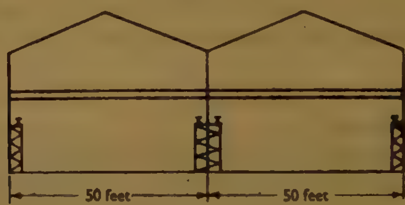
The Paper which had been presented that evening, however, was not a design Paper but one dealing with an analytical method, and there was a difference between design and analysis. The Authors and those associated with them were pioneering ahead, and practising engineers would be following in a more elementary fashion in the years to come. He doubted very much whether many practising engineers really understood the basic principles of Least Work, and he even doubted whether many of them were really capable of working out fundamentally the ordinary theory of deflexion of beams; for the most part when they wanted to know the deflexion of a beam they looked up a formula in a handbook. That evening, however, they had been listening to the basic principles of a plastic method of design, and he did not think it was going to be difficult or would take

long now to evolve a basis which the average practising engineer could understand and use.

The Admiralty had recently been considering a design for a workshop building 400 feet long by 100 feet wide, in two spans of 50 feet, which in section was as shown in *Fig. 22.*

Gantry cranes were required for the ground floor and the first floors had to be of concrete. An orthodox design was first made, consisting of

Fig. 22



Scale: 1 inch = 50 feet

simple roof trusses carried on light columns which were doubled up in the usual way to carry the gantry girders. The main 50-foot-span first-floor beams were 4-foot-deep welded made-up girders, and the total weight of steel came out at approximately 640 tons. While that design had still been in hand an advance copy of the present Paper had become available, and as a quick exercise a plastic portal analysis of a possible alternative design had been made (*Fig. 23*).

Fig. 23

Scale: 1 inch = 50 feet

When the main members were sized up as ordinary joist sections the total weight of steel was about 600 tons. If specially designed members had been used instead of standard joist sections, the weight could have been reduced a little, but not much. It had taken one man a little less than 1 week to do this—2 days to study the Paper and 3 days on the actual analysis.

Mr Little thought that the somewhat disappointing result—a saving of only 10 per cent in steel—was because the type and proportions of the building were not favourable to continuous members. The heavy fixing

moments in the main beams were carried through into the stanchions in the plastic design, but only direct axial loads arose in the orthodox design. A flat roof over the first floor would have made lighting more costly and, more important still, the columns in the plastic design would have been at least 3 feet deep ; and for the same span of gantry cranes the overall width of the building would have been at least 3 feet more. Finally, experience suggested that it might not be possible to get an all-welded design built at present, because when tenders had been invited recently from fifteen contractors for a 50-foot-span workshop, designed as a plastic portal, all had declined.

It was still difficult to make a fair comparison between the cost of welded and riveted work, and so assess the real economics of plastic designs. When tenders had eventually been received for the 50-foot-span workshop the price per ton had been about £98, but the cost for a similar building of 60-foot span (also a plastic design) had been £72 per ton. So far as overall costs were concerned, it was important to remember that, to get the best out of plastic designing, stanchion bases had to be fully fixed at ground level, and in bad ground the extra cost in foundations might more than offset the saving in steel.

Finally, for small buildings of, say, 30-foot span, to be erected in isolated places at home or especially abroad, it would not always be possible to get satisfactory site welding done. For such cases simple bolted connexions capable of developing the full plastic moments without yield in the bolts were required. Some small-scale experiments were being done on that as time allowed, but a determined attack on the problem should be made.

Mr F. Micklethwaite pointed out that in the design of many structures which had to be considered to-day many of the loads were reversing loads. That meant that it was necessary to consider what might happen under normal working loads, and with reversed plastic loads there might be fatigue troubles. How many reversals was it possible to take in the plastic range.

The Authors, in reply, emphasized the point made by Professor J. F. Baker and other speakers, that the method described in the Paper was in fact a method of analysis and not of design. There were, the Authors said, two aspects involved. In the first place the method itself only analysed a particular frame for plastic collapse. It was possible to start a design by assigning certain arbitrary ratios to the fully plastic moments of the members and to calculate the required values of the fully plastic moments by that method. It was then possible to investigate the effect of altering the sections of particular members to see whether it was possible to reduce the weight of the frame, but that was primarily an analytical approach. They were not aware, however, of the existence of any precise method of design, either elastic or plastic. Moment distribution, for example, was a method for analysing a particular frame.

Secondly, the method was only concerned with plastic collapse of a

frame as a whole, assuming that instability effects did not come into play. It therefore dealt with only half of the design problem. When designing elastically or plastically it was necessary to consider two problems: (1) the strength of the frame as a whole; and (2) the stability of individual members. The method described dealt only with the strength of the frame as a whole, and had nothing whatever to do with plastic stability problems, but they would like to point out that the latter problems had been under investigation at Cambridge for a number of years. Those problems were extremely intractable, but considerable progress had been made.

The Authors apologized for omitting from the Paper all those reserva tions on the method. They had been left out merely for the sake of brevity. In the past, the authors of Papers on the plastic theory which were concerned with the strength of frames as a whole had usually stated those reservations in their introductory remarks. However, no author of a Paper on a corresponding elastic problem—for instance, on moment distribution—would preface his remarks by stating the same provisos, and it was felt that those concerned with the plastic methods should have the same degree of freedom. The neglect of those effects was not a particular defect of the plastic approach, for they had to be considered even if a frame were being designed elastically.

It had been a little alarming to find Mr R. A. Foulkes introducing real loads and sections into the problem given in the Paper, because the Authors had merely given an idealized problem so as to explain their method without clouding the issue with complicated arithmetic. Mr Foulkes, however, thought that the load ratios bore some resemblance to a real problem, and by inserting practical values for the loads he arrived at the conclusion that the axial thrust in the centre stanchion would cause a reduction in the fully plastic moment of that member of about 30 per cent. That would probably be a common effect in multi-storey buildings. Though again they did not state that reservation in their Paper, the Authors had postulated the existence of constant fully plastic moments, whereas in practice a designer would assume a value for the fully plastic moment, calculate the axial thrust, and then apply an appropriate correction.

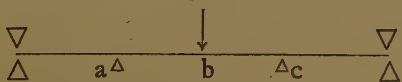
Dr Heyman had pointed out rather an awkward feature of plastic collapse analysis, which was that the principle of superposition did not hold. The Authors were glad that Dr Heyman had drawn attention to that difficulty. It was presumably a weakness of the plastic approach, but Dr Heyman had shown convincingly that it was quite simple to determine the worst combination of loads in a particular case.

Professor A. L. L. Baker had discussed the plastic approach to reinforced-concrete structures, which the Authors felt must be in many ways slightly different from the plastic approach to steel structures, because of the lack of ductility of reinforced-concrete members. The steel designer could always assume sufficient ductility in the members for the appropriate plastic hinges to form before fracture occurred. The concrete designer

could not make that simplifying assumption, but it was interesting to hear that Professor Baker had been carrying out some tests with spiral reinforcement, which seemed to increase the ductility of concrete enormously. Dr Neal had been interested in an effect of that kind himself, and it was known that surprising increases in the ductility in compression could be obtained by the use of small amounts of spiral reinforcement. It might be possible to use that type of reinforcement when constructing beams, so that a plastic design approach could be adopted.

Another point made by Professor A. L. L. Baker was that in certain type of structure extremely large deflexions might arise before the appropriate equalization of moments occurred. That was in fact true, and an old paradox on those lines had been stated by Stüssi. That was a continuous beam on four supports, the end supports being capable of preventing upward motion. A concentrated load was applied at the centre b of the central span ac as in *Fig. 24*.

Fig. 24



It would be concluded that collapse would occur when plastic hinges formed at a, b, and c, and that would give a certain collapse load regardless of the length of the outer spans. If the beam were made excessively long while the span ac remained constant, the collapse load would still have the same value, because there would be the same plastic hinges in the same positions. It would be found on examination, however, that before the hinges a and c would form, the deflexion beneath the load would become excessive, because the stiffness of the restraints at a and c would be very small. If the end supports were an infinite distance away, the stiffness of the restraints at a and c would become zero, and so Stüssi argued that the central span ac would in effect be simply supported. If that were the case, the collapse load would then be one half of its former value, although it was pointed out that the collapse load should be independent of the total span. The apparent paradox, however, was merely due to the fact that before the hinges at a and c were formed there would be infinite deflexions. It followed that in unusual structures it might be necessary not only to estimate collapse loads but to make some attempt to predict the deflexions which occurred on the point of collapse.

Mr Boulton had raised the interesting point as to whether the technique described in the Paper could be adapted to determine the relative ratios of beam and stanchion sizes which gave the minimum-weight structure, and he suggested a method of approach to that problem which was as good as any method which had been suggested to date. It would have been interesting if Mr J. D. Foulkes, who had spoken next in the Discussion, had dealt with that point, because Mr Foulkes had been investigating the

problem, and he had already obtained some interesting general results. It was possible that before long a systematic method for working down to the minimum-weight frame would be available. Dr Horne's approach might also be adapted to the solution of the minimum-weight problem.

Mr Foulkes had raised the extremely important point that whereas at some early stage of the analysis it might be found advantageous to bring a certain mechanism into combination, at a later stage of the analysis it might be found that that mechanism should be removed. Therefore, when what was thought to be the final solution was obtained, the analyst should investigate his solution to see whether a further reduction in W could be achieved by removing any particular mechanism. The method was not a method of analysis which could be turned over to a completely unskilled operator, and it would be appreciated that there was a definite technique involved. The Authors did not think, however, that that should lessen its appeal to engineers.

Dr Horne had raised the point that at any intermediate stage of the calculation the estimate of load was too high, and therefore represented an unsafe estimate. The Authors would counter that point by saying that a designer would never stop at an intermediate stage but would always obtain a precise estimate of the correct critical load. It should be added that, having obtained what was thought to be a final design, in which all the necessary adjustments for axial thrust had been made, and those members had been selected which were estimated to give the frame of minimum weight, the calculations should be checked by statics. That would be done by drawing the bending-moment diagram for the entire frame and ensuring that the fully plastic moment was not exceeded anywhere. That would provide a completely independent check on the calculations.

The alternative statical approach which Dr Horne had developed was indeed a complementary approach, as he had pointed out. It was quite possible that at a later stage those two approaches might be amalgamated with advantage to produce an even simpler method. So far, however, it was not possible to see how that could be done.

Mr Haythornthwaite had pointed out that there was no need to try every mode of collapse. The Authors endorsed that remark heartily; it was in line with the main object of the Paper. They were also grateful to Mr Haythornthwaite for drawing attention to some notions for avoiding the testing of certain combinations of the independent mechanisms. With very little experience of the method an operator would learn a number of short cuts to the final answer.

Mr Howells had mentioned the question of variable loading. That was another problem entirely: as the loads varied, would the plastic hinge rotations ultimately stop or not? If the plastic hinge rotations eventually ceased, the structure was said to have shaken down. The shakedown problem had been investigated at Cambridge and at Brown University in

America, and it could be treated by a technique very similar to that described in the Paper, provided that elastic solutions for the frame were available. It had been found that the loads above which a frame would fail to shake down were usually, in practical examples, only 10 or 20 per cent below the loads at which plastic collapse occurred. That figure of 10 or 20 per cent was based only on working out a small number of examples, and work on that problem was still continuing. The problem of shakedown was intrinsically more difficult than the problem of plastic collapse because, in order to obtain a solution, an elastic analysis of the frame was required.

Professor Pippard had asked how long it took to solve the problem given in the Paper. It had been set by Dr Symonds and it had taken Dr Neal 20 minutes to obtain the collapse mechanism shown in *Fig. 9* of the Paper, in which the effect of varying the positions of the plastic hinges in the beams had not been considered. Most of the time was taken up in sketching the mechanisms, and the calculations could almost be performed mentally. Although it was a simplified problem, so that the arithmetic was trivial, he would be surprised to hear of any elastic technique by means of which the same problem could be solved in a comparable time.

The Authors would not be drawn by Professor Pippard's tempting remark to the effect that the theorem on which the method was based was obvious. Their own feeling was that the theorem was not obvious. However, the theorem had been proved rigorously, and so the result could be used whether or not it was taken to be obvious.

Coming to the remarks of Mr Kibblewhite, the Authors said that they were glad that at least one member of the audience had understood the Paper, and assured him that the technique was in fact as simple as it seemed to be. Elastic deformations had not been ignored. The Authors were concerned only with the collapse state of the structure, and it could be proved that at collapse the increasing deformation under constant load was due solely to the rotations of the plastic hinges. That followed from the fact that at collapse the bending-moment distribution remained unchanged as the deflexions increased, and so there could be no change in the curvature at any cross-section other than at a plastic hinge. Their analysis was thus precise, and elastic deformations were not neglected. Test results bore out the theory with what Professor Baker had once described as almost indecent agreement.

Mr Little had spoken of an Admiralty design which used 640 tons of steel when designed elastically, and 600 when designed plastically. The main problem in that design appeared to be the design of the stanchions, which were subjected to very heavy axial loads, and so far plastic methods could not be used for stanchion design. It was therefore not surprising that only 40 tons of steel were saved in the design by using the plastic method. However, Professor J. F. Baker and his collaborators had recently designed a pitched-roof portal-shed type of building for the War Office. That building covered an area 420 feet by 208 feet, and when designed

elastically the total weight of steel was 181 tons, whereas the plastic design used only 118 tons. That was a spectacular saving, owing to the fact that the stanchions were not subjected to heavy axial loads.

Whilst it might at the moment be difficult to persuade contractors to use welding, it seemed probable that welding would be used more frequently in the future. Thus it seemed a little unfair to criticize the plastic method on the grounds that contractors were at the moment unwilling to erect welded structures. In conclusion, the Authors emphasized that the use of the plastic method undeniably resulted in a saving of steel. Bearing in mind the present acute shortage of steel, that would appear to be a great advantage which could outweigh many minor disadvantages.

WORKS CONSTRUCTION DIVISION MEETING

11 December, 1951

David Mowat Watson, B.Sc., Vice-President I.C.E., Chairman of the Division, in the Chair

The following Paper was presented for discussion and, on the motion of the Chairman, the thanks of the Division were accorded to the Authors.

Works Construction Paper No. 19

"The Construction of Kafr el Zayat Railway Bridge"

by

Kenneth Edwin Hyatt, B.Sc.(Eng.), A.M.I.C.E.

and

George William Morley, M.A., A.M.I.C.E.

SYNOPSIS

The Paper describes briefly the old Kafr el Zayat Railway Bridge over the River Nile between Cairo and Alexandria and gives a general description of the new bridge which was completed in 1949.

The programme of construction and the layout of the site and plant are discussed. The handling of the materials and concrete are briefly touched upon.

Details are given of the construction of the pneumatic caissons forming the piers of the bridge, together with a description of a novel method adopted for launching three of them. Special difficulties encountered in an attempt to sink two caissons in a very short time and the steps taken to protect incomplete caissons during the flood period are described. The removal of stone pitching and an old wrought-iron dolphin situated on the site of a third caisson provided an interesting problem. The large swing-pier caisson could not be sunk as programmed and the staging driven for this caisson was left in position during the second flood period. The results of this action and the additional complications caused by the sinking of this caisson in the following season are described. Notes are given of sinking rates, areas of working surfaces, and skin friction on caisson sides.

After a brief description of the granite piers and concrete copings, the Paper goes on to describe the erection of the steel superstructure. Four spans were erected on trestle falsework, one on timber piles, and the swing span by cantilevering in the open position. The seventh span at the east end of the bridge was built partly on the river bank and rolled and floated into position. The Paper gives details of the erection methods adopted and concludes with information regarding riveting and painting.

Introduction and Description of Bridge

The Kafr el Zayat Railway Bridge carries the main double-track railway line from Cairo to Alexandria over the Rosetta branch of the River Nile, approximately half-way between the two cities. This branch of the Nile

is about 500 metres wide at this point and the water level varies from a maximum of + 8·5 metres to a minimum of + 0·5 metres.

The existing bridge was constructed in about 1875 and consisted of thirteen spans of 39 metres, supported on wrought-iron and cast-iron cylinders taken down to the level − 18·0 metres. There was a swing span near the east end to allow for navigation. The bridge was of the multiple-lattice type with continuity over certain of the piers, but not over its full length. It carried a double rail-track and two 3-metre roadways on cantilevers outside the main girders which had been out of use for some years. The superstructure was in excellent condition, but the cylinders showed signs of severe corrosion and the whole structure was considered too weak for modern railway traffic. A speed limit of 5 miles per hour had been imposed for many years, thus delaying all trains on this important line.

The Egyptian State Railways decided to renew the structure and called for tenders in 1946 based on a design made in their own office. This design is for a steel bridge of seven spans, of which the second from the eastern end is a swing span with two arms, each 35 metres long, and the remainder are simple spans of 70 metres.

Trusses of the Warren type with sub-divided main panels and an overall depth of 9 metres have been adopted. The bridge carries a double track and two small cantilevered footpaths for pedestrian traffic.

The cross girders are at 7-metre centres and plated stringers are placed directly under the rails. The top flanges of these stringers are extended laterally to give room for bolts securing the timber sleepers, which are carried directly on the steelwork. The deck is open with timbered gangways for access and the footpaths are floored with timber.

The swing-span turntable is of the normal type and consists of upper and lower roller paths of cast steel, between which are tapered cast-steel rollers, 0·36 metres in mean diameter. A cast-steel centre pivot is provided to take lateral forces. There is no drum girder, the upper roller path being directly bolted to the main truss members; two heavy cross girders, and trimmer girders are arranged to give continuous bearing on this upper path.

A rack with moulded teeth is attached to the outside of the lower path. Two pinions are connected by trains of gears to the two 15-horse-power motors, and hand turning gear is also provided, with safety interlocks. The end wedges are also electrically and hand operated and are placed on the two supporting piers.

The control gear is of a simple type with a drum controller, but all movements are fully protected by limit switches and electrical interlocks. A position indicator is fitted in the control cabin built on a platform on the top booms at the centre of the swing span. The superstructure weighs 3,600 tons, with an additional 100 tons of turning gear.

The bridge is designed to the Egyptian State Railways "type D"

loading, consisting of two 100-ton locomotives with 80-ton tenders, followed by an 80-ton wagon; the maximum axle loads are 25 tons.

The site chosen is 42 metres south of the existing bridge and parallel to it. The foundations are built on caissons sunk by compressed air to a maximum depth of — 25·0 metres. Above zero level the piers are of mass concrete with a facing of granite ashlar masonry and with reinforced-concrete capping slabs. Small wing-walls are provided at each abutment and are carried on reinforced-concrete piles about 10·0 metres long.

Floating fenders are provided at the tower pier to protect the swing span when in the open position and to form walkways for the boatmen manoeuvring their craft past the bridge. These floaters consist of a series of riveted steel boxes connected together, in two rows 16·0 metres apart, and provided with cutwater sections up and down stream. They are anchored by chains to the river bed and guided by rails fixed up the face of the tower pier. The general arrangement is shown in *Fig. 1* and Figs 2, Plate 1, a photograph of the completed bridge in *Fig. 3*.

The tenders were opened in September 1946 but the adjudication was much delayed, and it was not until January 1947 that the contract was awarded to Messrs Dorman, Long & Co., Ltd., of Middlesbrough, England, at a contract sum of £561,070.

No particular difficulties were anticipated in the construction of the bridge, which is of a perfectly normal design in all respects, but problems arose during the course of the work and it is with these that the Paper is concerned. In addition to the engineering problems encountered, the site staff had to contend with difficulties caused by the cholera epidemic in Egypt in 1947 and the war in Palestine in 1948, as well as with the aftermath of the last war, such as the restricted shipping from the United Kingdom and the long delivery dates of many materials.

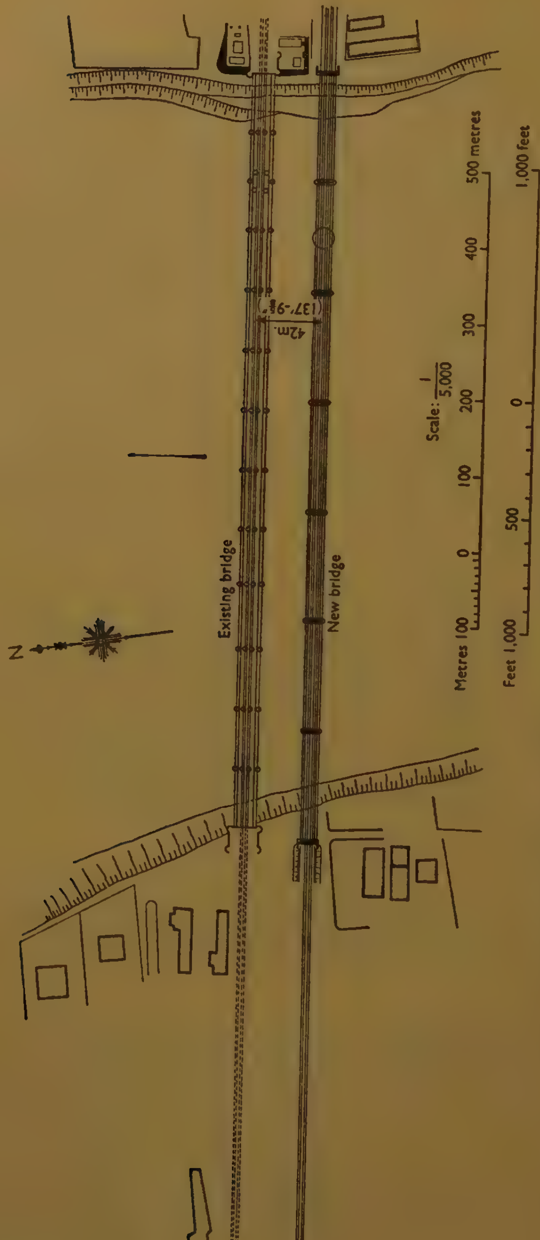
Programme

Although the contract was not signed until the 25th January, 1947, and it was not expected that site work could commence before April, it was still hoped to sink two caissons before the 1947 flood in August would stop work for the season.

The initial programme, therefore, was to sink caissons Nos 2 and 3 during the remainder of the 1947 dry season and abutments Nos 1 and 9 during the flood period. Caissons Nos 3–8 were to be sunk in the 1948 season, during which abutment No. 1 and piers Nos 2–5 were to be completed and the steelwork erected for spans Nos 1–4. In 1949, the balance of masonry and remaining three spans were to be erected, including the turning machinery, floating fenders, and all subsidiary work.

Owing to various delays outside the control of the contractors, caissons Nos 2 and 3 were not quite completed in the first season and the sinking of the swing pier No. 7 could not be carried out until the third season.

Fig. 1



PLAN SHOWING POSITION OF NEW BRIDGE RELATIVE TO EXISTING BRIDGE

Fig. 3

General View of Completed Bridge

Fig. 7

BUILDING CAISSON NO. 7 ON PONTOONS

Fig. 10

VIEW OF SITE FROM ROOF OF HOUSE

However, the first four spans were erected in 1948, as contemplated, and work was substantially completed by the contract date, July 1949. (Figs 4, Plate 1.) The installation of the electrical gear occupied a further 3 months, but that was additional to the original scheme.

Site Lay-out and Plant

Site-Lay-out

The lay-out of the site is shown in Figs 5, Plate 2. Office accommodation, stores, and living quarters were provided by an existing house and buildings on the west bank of the river. In addition, a cement store and power house were built and the area to be occupied by the approach embankments was used for material storage until August 1948. Since the areas available on the river banks were very restricted, full advantage had to be taken of the exposed river bed or beach at low Nile.

Power and Air Supply

The power house was equipped with three diesel generators of 110 horse-power and one of 85 horse-power, one being normally kept idle as a stand-by. These machines supplied current to three electrically-driven 300-cubic-foot compressors and the outside power lines. Power was supplied at 400 volts, 3-phase, and 50 cycles per second, and lighting current at 220 volts, except for the caissons at which transformers were installed to reduce the voltage to 25.

Electric cables were taken out along the old bridge and tapped off for each pier as required, and 4½-inch-diameter air pipes were run to caisson positions either on the beach or along the old bridge for both low- and high-pressure air. At a later stage, a generator and compressor were mounted on a barge for work on caisson No. 8 and then transferred to the east bank for caisson No. 9, together with a second generator, for use as an independent power station. Three large portable compressors were employed to supply high-pressure air for riveting and were also used as stand-bys for caisson air when the electric machines were hard pressed.

Sinking Gear

Two complete sets of sinking gear were provided so that two caissons could be sunk simultaneously. One vertical combined man-and-material lock was used for each caisson with "figure eight" shafting in 10-foot lengths. An additional man-lock was also provided but proved to be unnecessary. One 3-inch-diameter air connexion was made direct to the air lock and a second direct to the roof of the working chamber.

Cranes

For general service, a standard-gauge track was laid through the yard on the west bank and this was extended down on to the foreshore and exposed bed of the Nile during the dry season. The supporting earth

ramp and low causeway had to be re-constructed after each flood. Materials were delivered to site by both road and river and this track acted as a connecting link and construction track for all work between abutment No. 1 and pier No. 5. A 5-ton and a 3-ton loco crane operated on this track and were able to negotiate the maximum gradient of 1 in 18 without difficulty. An electric haulage winch at the top of the bank handled material on bogies on the ramp.

Normally, each caisson during sinking was served by a 5-ton and a 2-ton electric Scotch derrick. The erection of the steel spans was carried out by means of a 10-ton electric derrick with an 85-foot jib, specially adapted to travel forward on the superstructure. Unloading of heavy lifts was effected by a 10-ton hand crane. Hand cranes were used for building the pier masonry. Cranes were, in some instances, mounted on pontoons to serve piers Nos 6–8.

Other Plant

Four concrete mixers were employed on the work, two 21/14-cubic-foot electrically-driven machines and two of 14/10 cubic feet capacity with petrol engines. The latter proved to be insufficiently robust to give really satisfactory service although quite new.

A small petrol-paraffin tug and a 30-ton tank-landing craft were used for haulage and carrying on the river and fifty ex-army P.C. pontoon units provided a most useful means of constructing pontoons of different sizes for a wide range of duties.

Piling plant consisted of a standard steel 65-foot frame with a diesel-driven winch. There were diesel and electric pumps for water supply and portable fire pumps were used for jetting. Shore equipment included a band-saw and the usual workshop appliances.

Transport and Handling of Materials

Concreting sand and aggregate were delivered by road to the storage areas on either bank and all caisson material was delivered by road. Materials were distributed through the job on bogies from the yard and loaded, where necessary, into barges for work in the river. Granite for the pier facings was handled similarly, although this, in the first instance, had to be carted from the railway station where it was delivered from Upper Egypt. Cement was also received in this manner.

Fabricated steelwork for the spans was transported by barge from the port of entry, Alexandria, and unloaded in various positions on the site by any available crane.

Concreting

Concrete mixes were specified, varying from approximately 1 : 2¼ : 4½ to 1 : 1½ : 3, according to the position in the work. Two gradings of

gravel were used for heavy aggregate: 7–70 millimetres for mass concrete, and 7–30 millimetres for reinforced concrete.

The sand was required to be well graded from 0·5–7 millimetres, with not more than 20 per cent passing a 1-millimetre screen.

Washing of gravel and screening of sand, where necessary, was carried out at the dumps before delivery to the mixing stations by decauville skips. Considerable difficulties were experienced in maintaining the specifications for these materials.

Wells were sunk and pumps installed for supplying water for concrete mixing and gravel washing.

The cement was of Egyptian manufacture to British Standards Specification.

Concrete for piers Nos 2–5 was produced in the mixing station shown in Figs 5, Plate 2, shot into skips standing on decauville bogies, and so transported to within reach of the cranes serving the piers. The concrete was deposited from the skips where required by means of these cranes. Abutments Nos 1 and 9 were fed direct from the mixers standing alongside, and concrete was mixed on the stages in the cases of piers Nos 6–8.

Caissons

Supply and Erection

All the caissons consisted of a steel shell of $\frac{1}{4}$-inch thick plating, with mass-concrete filling. Eight of them were rectangular in section, 15·5 metres by 5·5 metres, with three vertical voids leaving the concrete walls 1·25 metres thick, whilst the tower-pier caisson was 12 metres in diameter, with a single central void and walls 2·75 metres thick.

The steelwork for all caissons was fabricated in a Cairo workshop from plain material shipped from Britain. A combination of riveting and welding was used for site erection—since it was easy to assemble the parts by means of the rivet holes, whilst the welding served the double purpose of strength and water tightness.

The cutting-edge sections were about 4·5 metres in height and were strongly braced with vertical steel frames. Above this section the shell plating was supported every 1·25 metre in height by temporary horizontal steel frames, which were removed as the concrete reached them. It was normal practice to build up the concrete walls to about 1 metre above the outside water or ground level before sinking was started and to maintain it above this level at all times.

Thereafter the function of the temporary frames was simply to maintain the shape of the caisson during building and concreting until the top of the concrete walls was reached. Temporary steel cofferdams were bolted to the tops of the shell plating during this period and maintained in position by means of more temporary bracing frames until the caisson was founded and completed and the pier masonry built up above the water or ground level.

The bracing frames, at this stage, were heavily loaded, in four cases to the extent of 4·8 metres of water and earth pressure.

All the rectangular caissons, particularly the small abutment ones, were short of sinking weight during the later stages. To help matters the voids and temporary cofferdams were filled with sand, excavated from inside the working chamber, and fully saturated with water pumped from the Nile. The connexions of the temporary bracing frames to the cofferdams had to be made sufficiently strong to withstand the bursting pressure of this sand filling.

The original intention had been to build caissons Nos 1–6 and 9 on the ground exposed at low Nile or on sand islands and caissons Nos 7 and 8 from piled stagings. However, owing to a change in the river bed from the profile shown on the contract drawings, a stage had also to be provided for caisson No. 6. The arrangement of the sand islands and stagings is shown in Figs 5, Plate 2.

Caissons Nos 6–8 were built on shore, or, as described later, launched and towed into position in the stages. Caisson No. 5 was also towed into position and the sand island for supporting the plant was formed round it after pitching. The remaining caissons were set up in the dry at each site.

After experiencing some trouble in launching caisson No. 6 down a slipway, caissons Nos 5, 7, and 8 were handled by a species of floating dock devised on the site and built from P.C. pontoon units. Six units were required for each ordinary caisson and eight for the swing-pier caisson. The lay-outs are shown in *Figs 6*. The cutting edges, working chambers, and necessary strakes of skin plating were assembled, riveted, and welded on the pontoon assemblies, which were then sunk by flooding. The caisson was floated clear and the pontoons blown up to the surface again by compressed air.

Caissons Nos 5, 7, and 8 were floated very satisfactorily by this method. The rectangular caissons each weighed 40 tons and floated in 1·9 metre of water, whilst the circular caisson weighed 55 tons and floated in 2·2 metres of water, in both cases with about 10 tons of concrete poured in the cutting edge to seal any leaks not accessible to welding. *Fig. 7* (facing p. 105) shows caisson No. 7 being built on its pontoons.

The river caissons were not supported directly by their stages, except for small lateral guides, but were positioned by single anchors upstream and also on each side while concrete was added to sink them to the river bed. This proved a perfectly satisfactory method in a current of about 0·5 metre per second and each of these caissons grounded in a good position. A reserve of buoyancy could be obtained by admitting air to the working chamber to allow the position of any caisson to be altered after grounding but this usually proved to be unnecessary. Men entered the working chamber as soon as possible to level the river bed before the cutting edge settled in fully.

The floating diagrams for caissons Nos 7 and 8 are shown in *Figs 8*. In these diagrams the depth of the caisson cutting-edge below water level is plotted against the weight of the caisson for various conditions of free floating, and floating assisted by air pressure in the working chamber. The weight of the caisson can be read off the curve showing the height of concrete poured above the roof of the working chamber, and the air pressures necessary to produce different conditions of internal water level can also be read from the appropriate lines.

The diagrams proved most useful in the control of the caissons in their early stages before grounding and before the weight became so great that all possibility of lifting the caisson by means of excessive air pressure could be discounted. The condition of caisson No. 8 when it was pitched, as described later, is clearly indicated on the diagram.

Sinking and Concreting (*see Tables 1 and 2, pp. 115–116*)

The air supply was cooled by passing it through a grid of piping kept moist by means of a small pump, but it proved very difficult to prevent the temperature rising again in the long leads from the compressor house to the caissons. A water trap was also employed but proved ineffective and, if the drains were not watched, enough water collected in the air pipes to stop the supply of air. The locks were protected by reed mats. Temperatures in the working chamber and main lock were 10° F. higher than outside shade temperatures in summer and the humidity was very unpleasant.

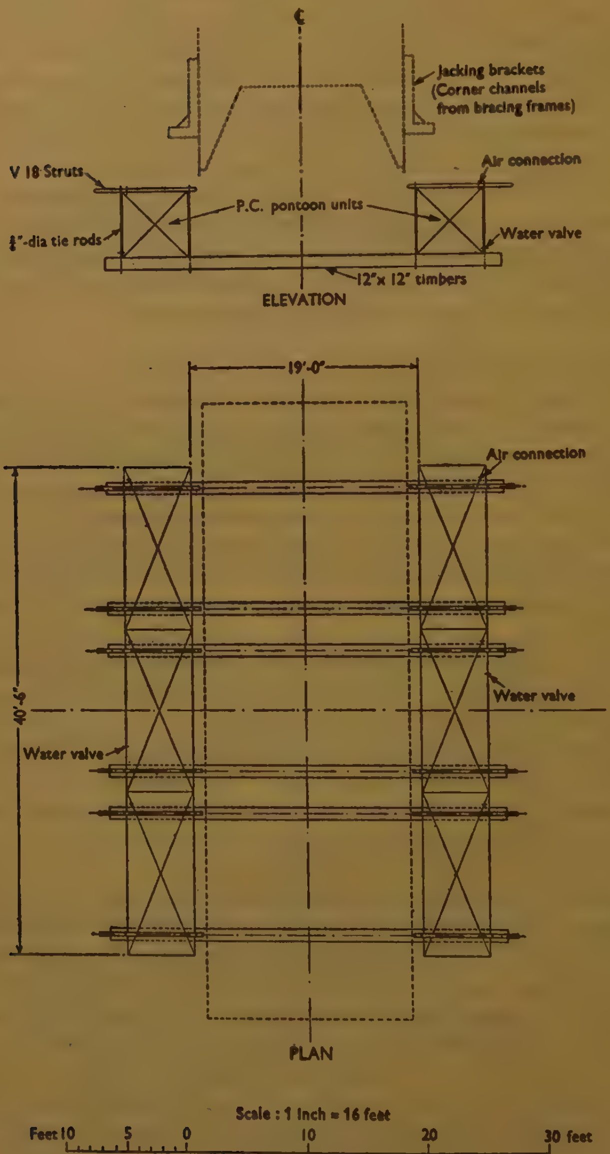
In spite of these conditions and rapid two-stage decompression from the maximum of 38 lb. per square inch pressure, cases of sickness were rare and the doctor's services were never required.

The normal sinking shift consisted of eight sinkers, one inside and two outside lock keepers, and a chargehand. This number was increased by two for caisson No. 7. The area of working surface per sinker was 10·6 square metres for the normal pier caissons, 7 square metres for the abutments, and 11·3 square metres for the swing pier.

The soil encountered was mainly silt and sand, but a bed of clay was met in caisson No. 9 and considerable quantities of stone pitching in caisson No. 8. The speed of sinking was controlled entirely by the speed at which the rising concrete could be poured, unless special measures had to be taken to correct a caisson for inclination or position. Only one shift per 24 hours was therefore normally worked, with one shaft per caisson even in the case of No. 7. At times excavation speeds of 20 buckets (capacity 0·6 cubic metres) per hour, or 160 per 8-hour shift, were maintained, corresponding to a solid excavation of 64 cubic metres, allowing 50-per-cent bulking. The best average was 45 cubic metres per shift for the whole of caisson No. 5. The recorded levels of the cutting edges of each caisson during sinking are shown on the caisson-sinking chart (*Figs 9*).

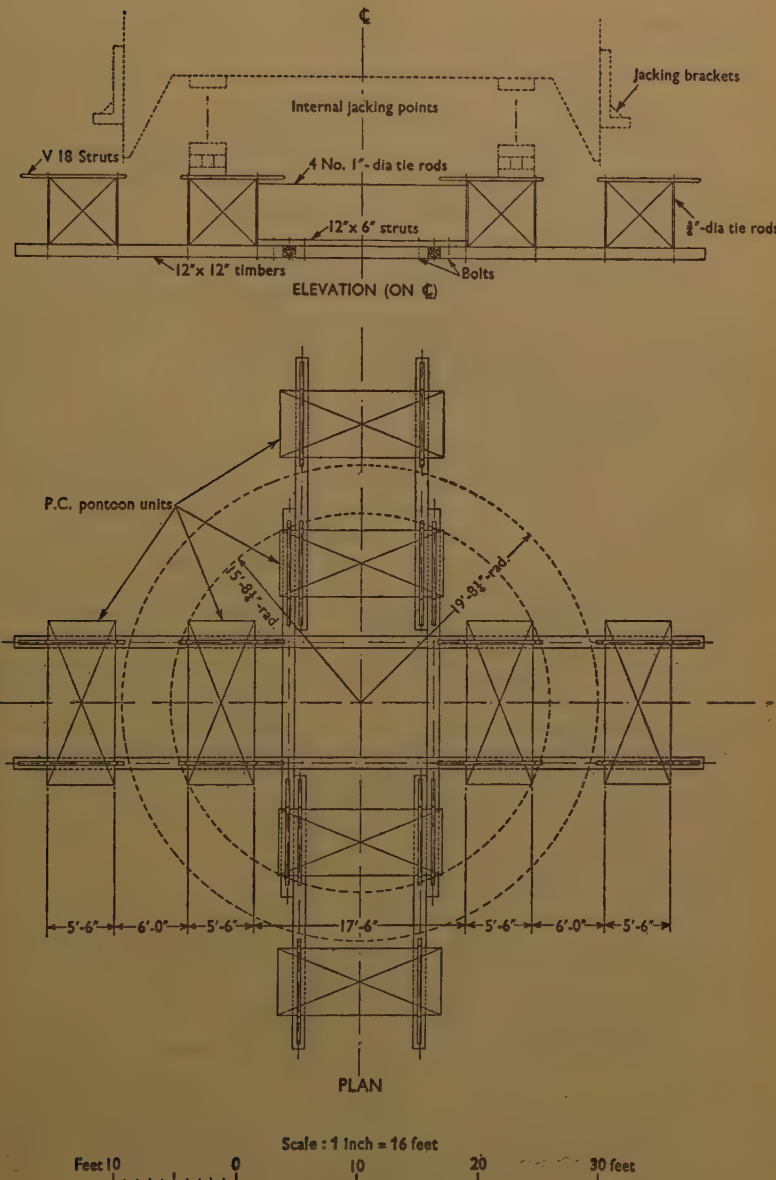
Excavated material from the caissons on the beach was used to form

Figs 6 (a)



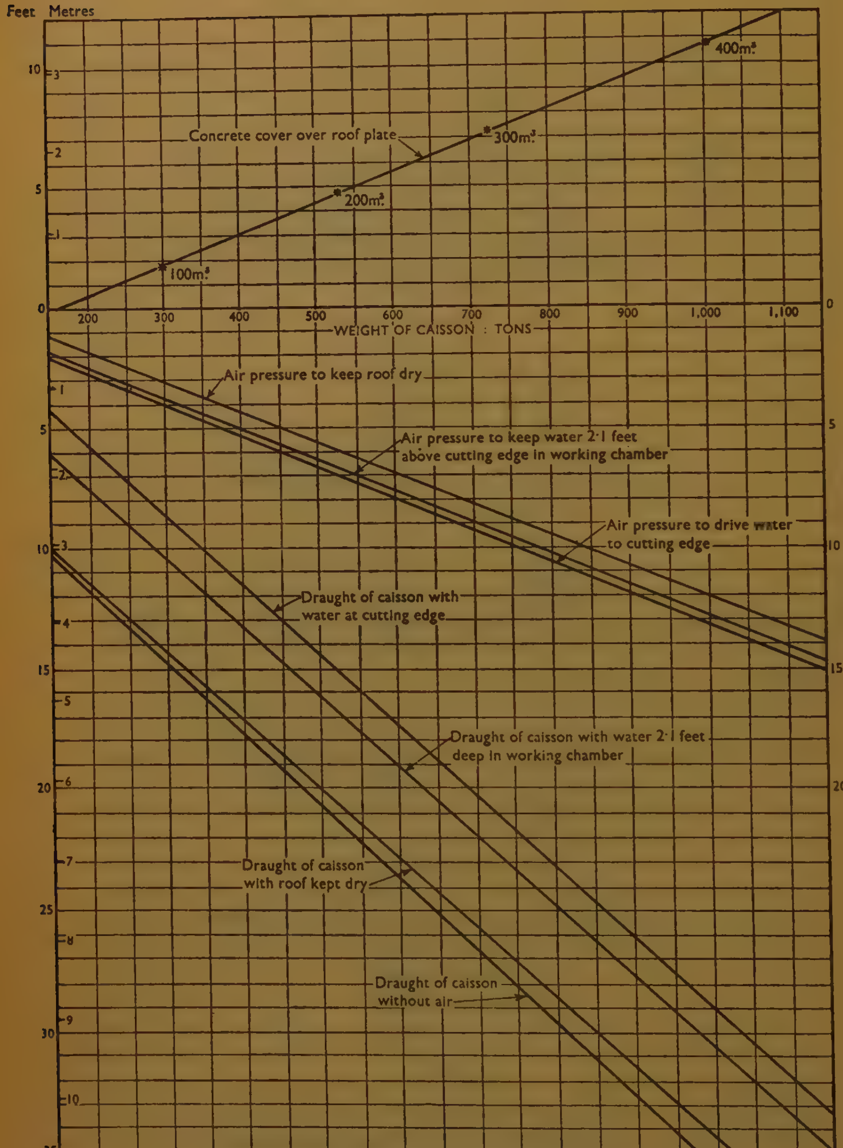
FLOATING DOCK FOR CAISSONS NOS 5 AND 8

Figs 6 (b)



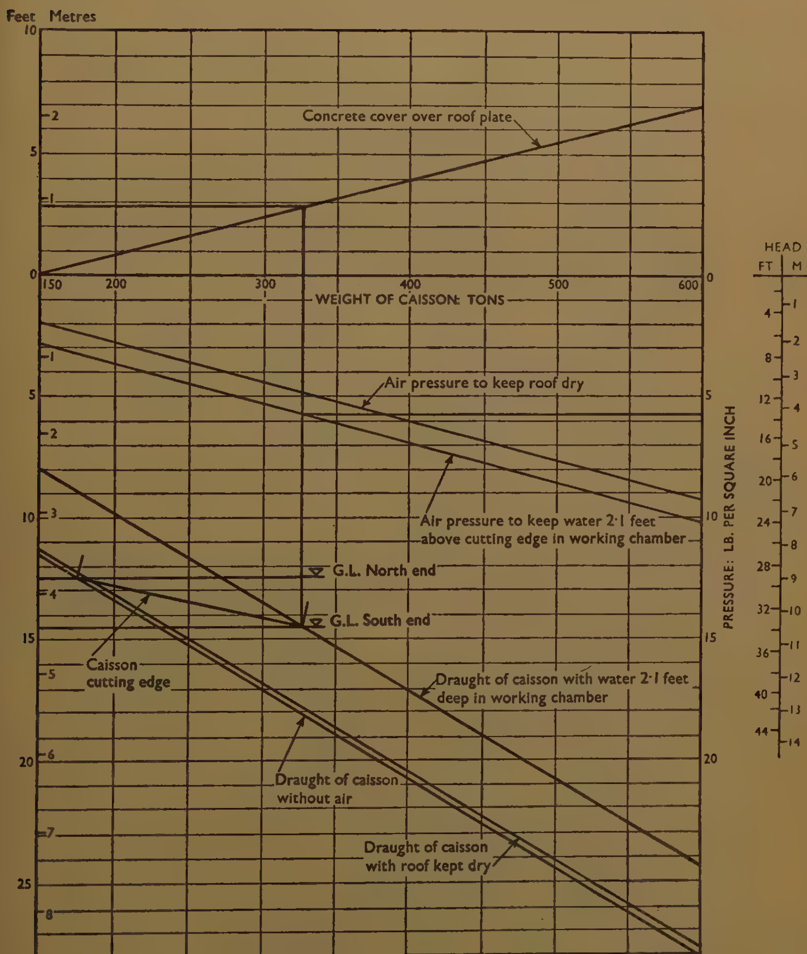
Floating Dock for Caisson No. 7

Fig. 8 (a)



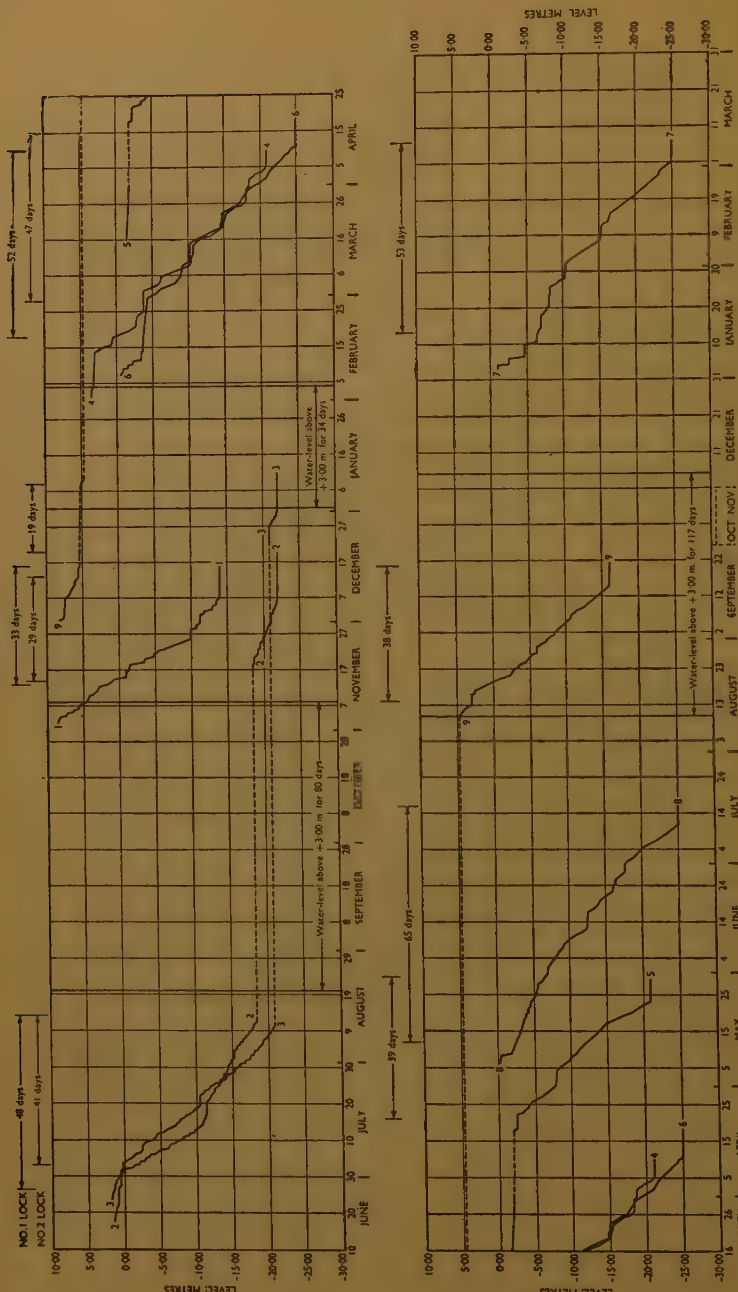
Floating Chart for Caisson No. 7

Fig. 8 (b)



Floating Chart for Caisson No. 8

Figs 9



CAISSON SINKING CHART

TABLE 1.—CAISSON CONCRETING RECORD

Caisson No.	2	3	1	4	6	5	8	9	7
Caisson area : square metres	85·25	85·25	56·25	85·25	85·25	85·25	85·25	56·25	113·15
Founding level : metres	−22·0	−22·0	−14·0	−21·0	−25·0	−21·0	−25·0	−16·0	−25·0
Closing slab level : metres	0·0	0·0	+ 4·0	0·0	0·0	0·0	+ 4·0	0·0	0·0
Original ground level : metres	+3·3	+2·3	+8·8	+0·8	−3·1	−1·5	−5·5*	+8·7	−6·5
Cutting-edge level at start : metres . .	+0·9	+0·3	+2·2	+0·5	−4·4	−2·3	−2·5	−3·0	−7·0
Water level at start : metres	+1·9	+1·8	+2·3	+0·5	+0·3	+1·6	+1·8	+5·9	+1·2
Water level at finish : metres	+2·8	+2·0	+2·1	+1·3	+1·3	+1·8	+1·7	+6·9	+0·2
Concrete quantity : cubic metres . . .	1,192	1,197	699	1,148	1,381	1,148	1,380	789	1,920
Number of shifts	23	21	17	21	24	25	23	18	25
Concreting rate per shift : cubic metres .	52	57	41	55	58	46	60	44	77
Maximum per shift : cubic metres . . .	80	79	55	76	79	76	85	56	103

* Round the existing dolphin cylinder there was hardcore pitching up to level − 1·5 metre.

TABLE 2.—CAISSON SINKING RECORD

Caisson No.	2*	3*	1	4	6	5	8	9	7
Air turned on (date)	26/6/47	3/7/47	13/11/47	18/2/48	28/2/48	21/4/48	12/5/48	14/8/48	13/1/49
Air turned off (date)	13/8/47	13/8/47	16/12/47	10/4/48	15/4/48	30/5/48	16/7/48	21/9/48	7/3/49
Days under air	48	41	33	52	47	39	65	38	53
Excavation shifts	120	112	37	45	54	38	118†	35	53
Concreting shifts	8	9	5	7	6	7	8	6	7
Special shifts	15	9	2	1	8	7	16	1	13
Solid excavation : cubic metres	1,640	1,810	912	1,873	1,850	1,705	1,960	1,069	2,078
Depth of sinking under air : metres	19·2	21·0	16·2	21·5	20·6	18·7	22·5	19·0	18·0
Concrete in working chamber : cubic metres	154·4	142·0	86·2	124·8	134·4	142·0	144·4	96·0	197·2
Excavation per shift : cubic metres	13·7	16·2	24·7	41·7	34·3	44·9	16·6	30·5	39·2
Sinking rate per shift : metres	0·16	0·19	0·44	0·48	0·38	0·49	0·19	0·54	0·34
Maximum per day : metre	1·50	1·00	1·90	1·20	1·20	1·10	0·90	1·10	1·00
Concrete per shift : metres	16·9	15·8	17·2	17·8	22·4	20·3	18·1	16·0	28·2

* The figures given for caissons Nos 2 and 3 refer only to the partial sinking accomplished during the 1947 season.

† This number of shifts includes ninety-two shifts during which the presence of the old dolphin cylinder and hardcore pitching greatly reduced the rate of sinking.

causeways and sand islands out to No. 5 pier and to form a bank for the trestle supports for span No. 4. In addition, 8,000 cubic metres of spoil was imported for these purposes from an adjacent sandbank.

In the first two caissons, the concreting was carried out as rapidly as erection of shell plates and shutters would permit, working day and night, but thereafter the Egyptian State Railways restricted the pouring of concrete to the period between 6 a.m. and 10 p.m. When placing by means of skips handled by the derricks, it was normally possible to complete a layer 1·25 metre thick during this time, but the contractor was not permitted, after caissons Nos 2 and 3, to raise either shutters or temporary frames until after 48 hours for ordinary and 24 hours for rapid-hardening cement. Rapid-hardening cement was used, giving one lift of concrete every 2 days, and the corresponding sinking rate was therefore 0·6 metre per day.

The internal shutters to the voids were composed of $\frac{1}{8}$-inch steel plate 1·5 metre deep. Since the voids were approximately oval in shape, these shutters were made in halves wedged out at the joints. Very little strutting was required and the shutters were raised bodily between lifts. The same shutters were modified for the swing pier, forming two lifts which were moved by leap-frogging.

Most of the sinking work was straightforward but in three instances some interesting problems were encountered.

Caissons Nos 2 and 3 (*see Fig. 10*, facing p. 105).—In an endeavour to make up for a late start during the first low Nile season, the construction of caissons Nos 2 and 3 were rushed, in order to try to complete the work in one month. Great efforts were required to obtain and ship the 150 tons of plates and sections in time, and the fabrication of the steelwork was so hurried that the workshops in Cairo were seldom more than two strakes of plating ahead of the site, which led to some difficulties and delays in erection. At one stage pouring of the concrete had to be stopped owing to the late arrival on site of the bottom shaft door and the consequent impossibility of extending the air shaft by changing the lock. Heavy blowing down had therefore to be resorted to without waiting for the building up of sufficient weight. Blowing down caused trouble and, although the major part of the sinking proceeded rapidly with the caissons maintaining a good position, they moved out of plumb when the penetrations were 12 and 19 metres into the ground, respectively. It had not been considered that such movements were likely at those depths and the race against the approaching Nile flood was virtually lost at that point.

It was eventually found impossible to beat the flood and sinking had to be abandoned with 2·7 metres to go in the case of No. 2 and only 0·3 metre for No. 3. The caissons were sealed up for 4 months by placing about 6 inches of concrete all over the floor of the working chamber, packing it well below the cutting edge, and then pumping clean water in through the air pipe in the roof of the chamber at a higher pressure than the air

supply. The air and water pressures were balanced as the water level rose and no settlement took place. The working chamber remained quite clear during the idle period but the start of the new season's work was delayed by the necessity of removing the concrete seal.

When the air was taken off at the end of the first season, the lock was removed and the shaft sealed with a blank flange, through which a small $\frac{3}{4}$-inch vent pipe was left open. The tops of the cofferdams in both cases were slightly below the original ground surface and were filled with sand so that little scour was anticipated. The only visible signs during the flood of 3 months' very hard work were two buoys marking the positions of the caissons!

Both caissons were re-started without difficulty in the following season and were taken 1 metre lower than the original founding level, which allowed their positions to be much improved.

Trouble was also experienced with No. 1 caisson due to blowing down too heavily.

Caisson No. 8.—Since one of the protective dolphins of the existing bridge was situated just inside the north end of the foundation area of this caisson, it was foreseen that problems would arise during sinking. This dolphin was one of a group of three which had been sunk about 1875 and about which no records existed. It was a cylindrical pier, 5 feet in diameter, with a $\frac{1}{2}$-inch-thick wrought-iron shell, in lengths of 2 feet 9 inches, the circumferential joints being covered internally by wrought-iron tee-bars. The top was at level + 9·0 metres and was plugged with concrete.

This concrete, which proved to be 2 feet thick, was removed with a breaker, and below the plug the cylinder was found to be packed solid with hard brick rubble, run in with a weak cement mortar. This material was very hard to remove in the confined space of the cylinder and the concrete breaker had to be used continually.

The cylinder, above water level, was cut into sections about 5 feet long with an oxy-acetylene burning torch and removed by crane. Excavation was then continued inside the cylinder to a depth of about 15 feet below the water level, by which time the leakage of water upwards from the bottom was becoming excessive. The heads of the rivets in the lowest circumferential joint were therefore burnt off and the rivets punched out against the external water and earth pressure. The inrush of water was immediately stopped by the insertion of plugs made from short pieces of old rubber hose with small tapered wooden plugs inside. When all rivets had been removed, a 200-ton jack was put into operation to shear the plugs and lift the cylinder against the external skin friction. It is estimated that a force of 50 tons was required to do that. The cylinder was then removed by the floating crane.

The dolphins had been surrounded with large pitching stones, dumped

year by year, until they reached a level at the cylinder of −1·5 metre, compared with a general bed level of − 5·5 metres. This pitching thus formed a hill which would not only prevent the caisson from floating into place but would not allow its being sunk to the river bed on an even keel. "Skin" divers were put to work and removed a great deal of the highest part of the pitching, whilst sand and rubble were tipped at the south end of the caissons. By this method the site of the caisson was improved until the level at the north end was about − 2·5 metres and at the south end − 3·0 metres; it was decided to ground the caisson at this stage.

The caisson was floated into position and concreting commenced. The north edge landed on a high spot at − 2·0 metres and the south end at − 2·7 metres. The caisson remained at this angle while sufficient concrete was placed to enable air to be turned on.

With the pressure at 5·5 lb. per square inch, the air was escaping under the cutting edge at the north end whilst the buoyancy was nearly sufficient to lift the south end again (see *Figs* 8), but the men were able to enter and remove the pitching at the north end to allow the caisson to sink. Subsequent sinking through the pitching proved to be a very slow process, the speed of sinking being 0·1 metre per shift only, as against a normal speed in sand approaching 0·6 metre. The caisson was maintained at a slope of about 30 centimetres from north to south until the original river bed was reached to ensure that the air would blow out among the pitching at the north end and not through the made-up ground at the south end, which might have carried away.

The sinking was further hampered by the presence of the lower portion of the dolphin cylinder because this had to be dug out and burnt off in sections as the caisson sank. This cylinder was found to extend to − 18·0 metres and was cut circumferentially twelve times and further cut up to pass through the lock. Each cut occupied one shift because the fumes were so bad that the men were unable to remain in the chamber for more than ½ hour at a time. Only at the last cut, made in a pressure of 28 lb. per square inch, was any difficulty experienced in using the oxy-acetylene torch in compressed air. The effect of the presence of the cylinder was to retard the sinking rate, after the pitching had been left behind, to an average of 0·3 metre per shift.

Caisson No. 7.—The building of this caisson was delayed since the ship bringing some of the materials was detained for 5 weeks at Haifa, owing to the Palestine troubles. An attempt was made to build it in time to sink at least 12 metres during the 1947 season, this being the minimum safe depth at which it was thought it could be left during the flood. It had been built and floated by the 27th June, when information was received that the flood was expected to reach the site by the 1st August—about 14 days earlier than normal.

The caisson was therefore secured to the new bridge and left floating until January 1949, and the staging piles and bracings were allowed to

remain in position marked by buoys. The stagings were secured by wire ropes and anchors.

The 1948 flood was a moderate one, with a maximum river level of + 7·45 metres, but it was prolonged later than normally. No evidence of damage to the staging by scour was noted until the 22nd November, 1948, when the river had fallen to + 4·10 metres. Then two piles, in the group of four which were to support the 5-ton derrick mast (see Figs 5, Plate 2), floated up, but were held by the wire. The flow of the river was still too great to measure the depth of water at the piles. On the 14th December, when the level was + 1·73 metre, it was found that the general level of the river bed had been lowered about 2·5 metres, the scour at individual piles varying from 1·2–3·8 metres. Since the original penetration had only been about 5 metres, it was clear that all the main crane piles would have to be re-driven at least 2 metres further. This held up the start of work on the caisson, and it was not until the 4th January, 1949, that concreting was able to commence, about one month later than had been hoped.

The caisson was sunk to the river bed in the manner previously described and made contact along the north-east arc on the 10th January. Concreting was continued for a further 2 days and on the 13th January the air was turned on to level up the river bed. Unfortunately, the weight of concrete deposited in this period was overestimated by some 110 tons and the air pressure caused it to float clear, except for the contact along the north-east arc. It swung rapidly 30 centimetres out of position.

This was corrected and thereafter sinking continued in the normal manner, and without any special points of interest until the caisson was approaching the founding level.

This caisson was the only "heavy" one, and the only one for which special precautions had to be taken during the final clearing out and concreting of the working chamber. It was estimated that the weight of concrete alone, reduced by the effect of buoyancy, would be more than sufficient to overcome the probable skin friction and would cause the caisson to sink beyond its founding level as soon as the chamber was fully cleared out.

The weight was therefore reduced as much as possible by omitting the last 2 metres of concrete in the walls of the caisson until after founding and by removing all sand and water from the void. In this condition, it was found to be just balanced by the skin friction, a reduction of air pressure by ½ lb. per square inch being sufficient to cause it to sink.

Skin Friction

The skin friction on the sides of caissons during sinking is difficult to measure with any accuracy. When caisson No. 3 was at a depth of 19 metres in the ground with the water level only 0·5 metre below the surface, the estimated dead weight was 2,830 tons, and the caisson was almost

level. The working chamber was cleared of sand to a depth of 12 inches below the cutting edge. The air pressure was then reduced from 26·5 to 22 lb. per square inch, when the caisson began to sink. It sank 0·75 metre without any further reduction in air pressure. The uplift due to 22 lb. per square inch of air pressure was 1,300 tons, and it was estimated that the skin friction was 3·5 cwt. per square foot at the commencement of sinking.

A similar measurement was made in caisson No. 1 at a penetration of 12 metres in the ground, but with the water level 6 metres below the surface. In this case the skin friction was 4·6 cwt. per square foot.

The skin friction on caissons Nos 4 and 5 at completion of sinking appeared to be 4·9 and 4·3 cwt. per square foot, respectively, whilst the balanced position of caisson No. 7, described previously gave a value of 4·3 cwt. per square foot. Since both Nos 1 and 3 were somewhat distorted in shape and had ordinary cup-head rivets on the outer faces, whereas Nos 4, 5, and 7 were in fairly good shape with rivets flattened to $\frac{3}{8}$-inch, there is no evidence that the smoothness and regularity of the surface had any beneficial effect on the ease of sinking in sand and silt.

Piers and Abutments

The shafts of the piers and abutments were of mass concrete, faced with grey granite from Aswan to an average thickness of 0·7 metre; the depth of courses was usually 0·4 metre. The tops of the piers were formed by reinforced-concrete capping slabs, with moulded cornices carrying the bridge bearings. In the case of the swing pier, the capping slab was massive because it had to span the central void which was a feature of this pier.

The temporary caissons were removed simply by unbolting the angle ring connexions to the permanent shells, flooding, and lifting away in sections by means of a crane.

The building of the reinforced-concrete wing walls on their piled foundations was entirely normal and calls for little comment. A bituminous construction joint was made between each wing wall and its abutment to permit movement which might arise from unequal settlement.

Erection of Steelwork

It was intended that the western spans, Nos 1–5, should be erected on falsework by means of a 10-ton electric derrick travelling on their top chords. The programme allowed for the erection of spans Nos 1–4 during the 1948 low-Nile season, and the fifth span was to follow in 1949. In the interval span No. 7 was to be assembled ready for combined rolling and floating into position on the first low water, after which the plant would

be transferred to span No. 5. Thereafter the derrick would be placed on a pontoon to carry out the erection of span No. 6 in the open position. In general, this erection scheme was carried out as intended but modifications were made as work proceeded.

Span Nos 1–4 (*see Fig. 11*).—The falsework for these spans was constructed from ex-Army V-type trestling, sufficient units being provided for the support of two spans simultaneously. The usual arrangement is shown in Figs 12, Plate 2. The trestles were normally founded on footings of weak concrete, 12 feet square and 6–9 inches thick, placed on the dry river bed with little excavation. In the case of span No. 1, the trestles were based on sleepers placed directly on the sand, and for span No. 4 the original ground level was made up to just above the lowest water level with spoil from caisson No. 5, on which the concrete footings were placed. No trouble was experienced in the latter case, although the river rose above the footings, but small settlements did occur in the case of span No. 1.

As erection proceeded, daily records of the camber were taken and small adjustments made by means of 120-ton hydraulic jacks placed where necessary on the trestle tops. Steel erection was commenced at the landward end of span No. 1.

After erection of the first four panels of span No. 1, by means of the 10-ton derrick standing on the ground, the derrick was moved on to the steelwork. It was arranged, however, to run at deck level instead of on the top chord. This enabled the kingpost to be placed on the centre-line of the bridge and equalized the loads imposed on the trestle foundations under each truss. As originally arranged, with the kingpost over one truss, unequal settlement might have occurred between trestle foundations.

A sleepered track, upon which ran a bogie carrying the mast of the derrick, was laid forward on the two centre stringers as work proceeded. One leg of the derrick was carried on another bogie trailing behind the mast. The sleeper beam of the other leg was replaced by two "V" struts, connected to the truss verticals when the derrick was in the working position, the corresponding raker being cut at a convenient splice and attached to the top chord of the south truss by a special bracket which was made to grip the member.

When in action the derrick mast was always situated 1·5 metre behind the last cross girder erected, from which position the whole of the succeeding two panels could be built. The sway frame immediately in front of the derrick at each point was always in position and fully bolted while lifting, because calculation showed that a considerable lateral force would be placed on the structure when the crane was lifting over the side in a moderate wind.

The same set of sway bracing was always carried forward with the derrick, being handled by means of a tackle attached to the mast head. As the derrick moved, the sway frames behind were erected by a timber stick attached to the trailing leg. Temporary guys were taken from the

Fig. 11

Completing Span No. 3

Fig. 14

Rolling Carriage for Span No. 7

Fig. 15

Floating-out Span No. 7

Fig. 16

Erection of Swing Span No. 6

mast head to suitable anchorages clear of the bridge as a safety measure during travelling.

Although the operation appears a little laborious, it proved simple enough and, after the men had become used to the drill, a cycle of erection of two complete panels of the bridge with one move of the derrick was regularly carried out in 5 days.

When passing from one span to the next, there was no interruption in the sequence of operations, but it was found convenient to use a pair of temporary trestles placed about panel point L_1. This was to enable the derrick to erect the first short portion of bottom chord of the next span and the end posts, while standing just behind L_8 of the previous span. Without this extra trestle, erection of the end posts of the next span would have been difficult.

Owing to transport difficulties, steel deliveries to the site were delayed from time to time and erection of the first span was not commenced until 6th March. Other delays were experienced but span No. 4 was completed by the 26th July, 5 days before the target date. This last span was erected in 19 days during the fast of Ramadan. Since the Irrigation Department expected the flood to reach Kafr el Zayat on the 1st August, this period was a very anxious one for the contractor.

Riveting followed closely behind the erection, all joints being riveted as soon as convenient, provided that they were properly drifted up and the camber was correct. When the main trusses had been completely riveted, the spans were lifted off the camber blocks on the trestles, by means of four 200-ton hydraulic jacks placed under the end cross girders, and lowered on to their permanent bearings. On span No. 1, riveting of the deck and laterals was carried out parallel with that of the main trusses, but this delayed the striking of the camber blocks, and for the other spans only the main trusses and top laterals were riveted during erection. This enabled the riveting of the main trusses to be kept so closely behind the erection that the camber blocks of span No. 4 were struck on the 1st August, only 5 days after completion of erection (see Figs 4, Plate 1).

Span No. 7.—The method adopted for the erection of this span is shown in Figs 13, Plate 2. Trestles were provided on the river bank under panel points L_6, and the amount of kentledge required was kept to the minimum by erecting the 10-ton electric derrick on trestles beside the span instead of on the top chord.

Theoretically the kentledge could have been omitted altogether for this method of erection, but it was decided to limit the load on the trestles to 100 tons per truss by placing 30 tons of sand on the deck between panel points L_0 and L_1. This kentledge was later increased to 120 tons when it was found that, owing to an unexpected change in the river level, the toe of the bank had to be excavated by grab to allow the floating-in of the pontoons which were to be used to carry out the span. This excavation brought the clay bank to the limit of its stability, and the additional

kentledge ensured that the span would have been safe even if it had slipped. In fact, no slip occurred and the trestles took their full calculated load, the span attaining almost exactly the calculated droop in the erected position.

For the floating-out of the span, all available P.C. pontoon units had to be assembled into one large pontoon, 67 feet 6 inches by 55 feet in size, capable of carrying a load of 365 tons with a freeboard of 6 inches. A four-legged trestle was built up of " V " units, the loads being spread over the pontoons by means of 22-inch-deep grillage beams, all arranged as shown in Figs 13, Plate 2. This pontoon was to be floated under panel points L_8 and L_9 of the span and was to carry a load of 306 tons, the remainder of the weight of the span being carried by E.S.T.B. joists and rollers under panel point L_0, running on 75-lb. rails on the river bank. Since one corner pontoon unit had to be omitted in order to avoid fouling a dolphin of the old bridge, and since the units were not all entirely water-tight after their extensive use on the contract, it was decided to reduce the load that they were expected to carry to about 295 tons by omitting all the walkways and stringer bracing between panel points L_4 and L_{10}, and also the end portions of the top chords U_9–U_{10}, until after the span had been floated out.

The pontoon was sunk to a freeboard of 6 inches by completely filling thirty-four units with water. In this condition it was floated into position below the span, and hardwood packs were inserted between the trestle tops and panel points L_8 and L_9. The water was then removed from the thirty-four units, by which time the pontoon had lifted the end of the span and had freed the trestles at L_6, but not the packing at L_4. These were quickly freed by hydraulic jacks at L_4, and the pontoon again sank until the freeboard was about 6 inches. It had been expected to float at this stage with a freeboard of 10 inches, allowing 5 inches of water in each unit, but it was found difficult to keep the water down in some of the units and the weight of water and gear carried must have been more than was anticipated.

The arrangement of stay wires and haulage ropes is shown in Figs 13, Plate 2. Although jacks were provided to assist in starting the movement of the span, they were not used. Most of the load was taken by the rear winches, which were working to the limit of their capacity. This was due to the sinking of the sleeper tracks under the rolling carriages.

The span was erected and floated out about 2 feet 6 inches above its final level—the height necessary to allow the carriages (*Fig. 14*) to pass over the bearing slabs. It was landed on steel joists and plate packings over the fixed bearings on pier No. 8, and was jacked down later, using four 200-ton hydraulic jacks placed at jacking points under the end cross girders.

Erection was commenced on the 1st December, 1948, immediately after receiving the steelwork from England, and the span was completed

and riveted ready to be floated out by the 29th January, 1949. At this period of the year the river level usually falls at the rate of about 20 centimetres per day. Arrangements were therefore made with the Irrigation Department to try to maintain a constant level for the 3 days, the 3rd, 4th, and 5th February.

The pontoons and trestles were assembled and ready for use by the 4th February, but work was held up on the 4th and 5th February by a high wind which caused waves to break over the edge of the pontoon and made it unsafe to lift the span. This wind fell slightly by the following day, and the whole operation of picking up the load of the span, floating out (*Fig. 15*), lowering on to packings, and removing the pontoons, was completed in 14 hours on the 6th February.

Span No. 5.—Owing to a shift in the river bed, there was too much water under span No. 5 to found the supporting trestles in the usual way, and they were therefore carried on groups of timber piles taken from the pier stagings. Also, the 10-ton derrick was erected on a pontoon instead of being remounted on the steelwork.

This change in plan resulted in some very neat timing of the use of the pontoon units, trestles, and other plant to work in with the launching and the completion of span No. 7 and of pier No. 8.

The pontoon units, generally in use as two or more floating platforms, and all the trestling had to be assembled for the span-No. 7 operation and then again broken up. As an illustration of the ease in handling of the pontoon and trestle units, it may be mentioned that after floating-in span No. 7 on the 6th February, the trestles were dismantled by the 10-ton electric derrick, which was then in turn erected on its pontoon ready for work by the 12th February. The previously assembled falsework trestles for span No. 5 were then placed in position and erection commenced as soon as the first consignment of steelwork, which arrived on the 13th, had been unloaded.

Span No. 6.—The turntable castings and centre portion of the steelwork of this span did not reach the site until the 2nd May, and the concrete coping slab of pier No. 7 was completed on the 4th May. The setting of the pivot pin was carried out on the 6th May, and thereafter assembly of the turntable and erection of the steelwork proceeded without interruption.

The floating crane was able to move around the pier as required, and extracted all the piles of the pier staging during intervals in the erection and riveting of the turntable. The span was built in the open position by cantilevering out each side of the pier (*Fig. 16*), a careful check of the panel point levels being taken whenever a balanced position was reached. All the bottom chord connexions, except the joint nearest to the swing pier, were riveted and the top chord connexions were fully bolted and drifted as erection proceeded; but the deflexion at the ends of the cantilever arms, when the steel erection was complete, was found to be about $2\frac{1}{2}$ centimetres greater than the calculated amount.

The span was swung round to the closed position and jacks placed under the end cross girders. An upwards deflexion of about $2\frac{1}{2}$ centimetres was given to each end of the span. With the span held in this position, the drifts were removed from each connexion in turn and replaced by rivets. Finally, before dealing with the last joint in the bottom chords, the jacks were released and the deflexion checked. It was found that the deflexion was still about 6 millimetres greater than expected and this was corrected by allowing these final joints to open $\frac{1}{16}$ inch, or the full clearance allowed by the service bolts.

A test load of sandbags was placed on the span and the deflexion was found to agree closely with the calculated amount, thus demonstrating that the operation of the wedges would be satisfactory. The final wedged level of each end of the span had been previously agreed with the Egyptian State Railways, and the wedges were then set to lift the span to these levels, when operated to the normal final position. By screwing up the wedges to the fullest extent, a further 6 millimetres of lift could be obtained if desired, whilst there was a clearance of 8 millimetres for swinging the span under the worst designed conditions of temperature.

Floating Fender.—The 127 tons of steelwork in the floating fender were fabricated in England and shipped completely knocked down to flat plates and sections. The sixteen floater boxes were assembled, riveted and tested on the beach from which they were launched and assembled with all bracing, fixings and winches in eighty days.

Riveting and Painting (see Tables 3 and 4)

Some notes on riveting and painting are given because these are operations in which practice and performance vary a good deal in different parts of the world. In the case of painting, doubts are often felt regarding the covering power of normal paints; details are given in Table 4.

All riveting was carried out by a local sub-contractor, who provided the labour, whilst the main contractor provided all staging, tools, and compressed air. All rivets in the spans were of $\frac{7}{8}$ inch diameter and of an average grip of about 4 inches. The average number driven per day of 9 hours was 290 per squad, with a maximum of 504. The same sub-contractor also carried out the riveting of the caissons and floaters, where, with $\frac{3}{4}$-inch-diameter rivets, his squad averaged about 410 per day and reached a maximum of 870. These numbers were only obtained by the payment of a good price, which provided sufficient incentive both to the sub-contractor and to his men, for, without this incentive, the numbers fell off very rapidly.

The steelwork was painted in the shops with one coat of red lead, and this was followed up on the site with a further coat of red lead and two coats of grey oil paint. The red lead paint was mixed on site immediately before using in the proportions of 72 per cent red oxide to 24 per cent linseed oil and 4 per cent turpentine, by weight.

Table 3.—Riveting Record

(*a*) *Span Rivets* ($\frac{7}{8}$ *inch diameter*)

Span No.	1	2	3	4	7	5	6	Total
Number of rivets driven	38,870	39,382	39,382	39,382	38,805	39,345	35,805	270,971
Number of days riveting	78	51	52	53	66	52	71	423
Number of gang-days	161	144	135½	133½	114	113½	135	936½
Rivets per gang-day	242	274	291	295	340	347	265	290

(*b*) *Caisson and Floater Rivets* ($\frac{3}{4}$ *inch diameter*)

	Caissons (nine)	Floaters (sixteen)	Total
Number of rivets driven	174.939	69,064	244,003
Number of days riveting	—	81	—
Number of gang-days	431	164	595
Rivets per gang-day	406	421	410

TABLE 4.—PAINTING RECORD

Note.—The swing span has been omitted from this record since the amount of paint used is affected by the areas of castings, machinery, control cabin, etc., which were painted.

Weight of steelwork (six spans): tons	3,163
Estimated area of painting: square metres	32,000
Painting area per ton: square metres	10·1
Red lead	
Estimated quantity of red lead paint required*: gallons; kilogrammes	631; 8,266
Actual quantity of red lead paint used: gallons; kilogrammes	555; 7,268
Covering capacity*: square metres/gallon	53·1
Undercoat	
Estimated quantity of second coat required: gallons; kilogrammes	492; 3,641
Actual quantity of second coat used: gallons; kilogrammes	528; 3,830
Covering capacity: square metres per gallon	60·7
Finishing coat	
Estimated quantity of third coat required: gallons; kilogrammes	376; 2,896
Actual quantity of third coat used: gallons; kilogrammes	483; 3,722
Covering capacity: square metres per gallon	66·3

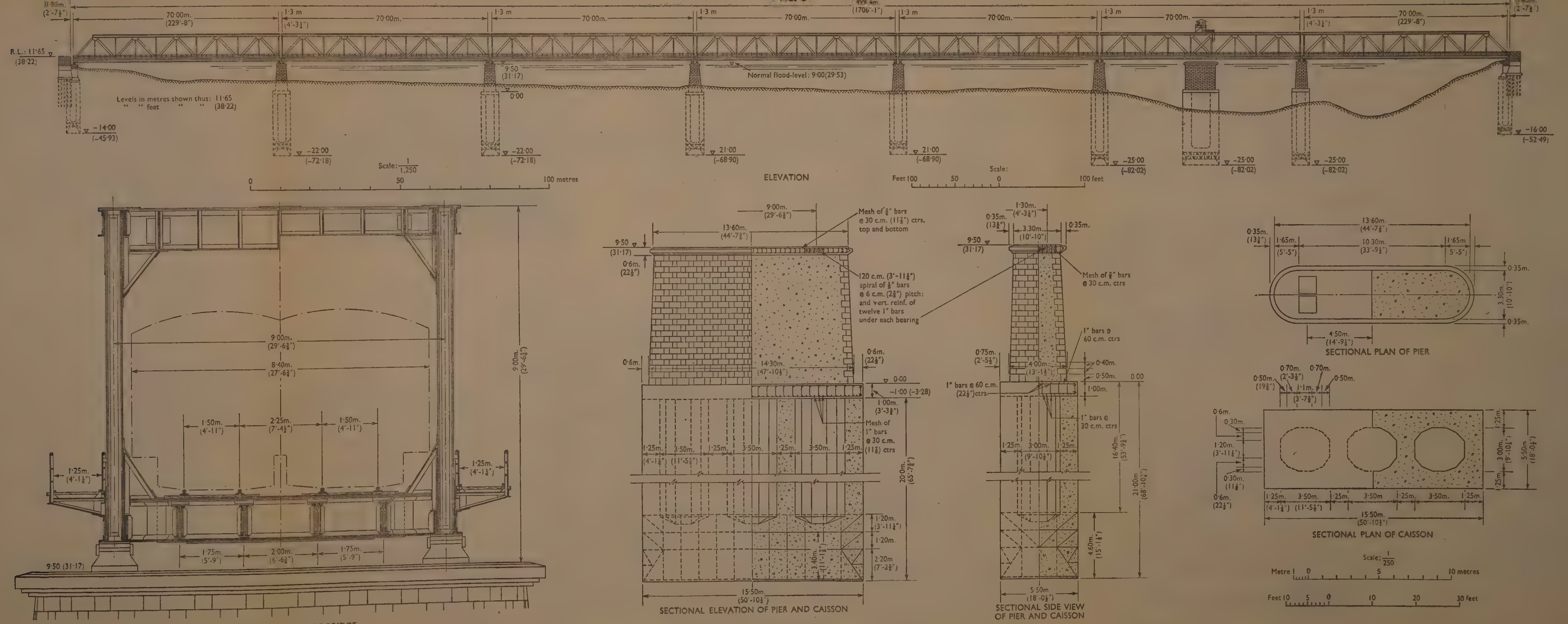
* Allowing 8½ per cent extra area for touching up damaged shop coat and for gusset plates, etc.

The application of the paint was let out to a local sub-contractor who had to carry out his own staging for this purpose. The cleaning and painting of each span took about 5 weeks, of which 3 weeks were occupied in cleaning, touching up, and painting with red lead, and 1 week for each of the two subsequent coats. The average number of men and boys painting was 17. The covering capacities actually measured were 53·1 square metres per gallon for red lead, 60·7 square metres per gallon for the under coat, and 66·3 square metres per gallon for the finishing coat.

ACKNOWLEDGEMENTS

As already stated, the work was carried out for the Egyptian State Railways. Dr Sayed Bey Abdel Wahid, who is now the General Manager to the Railways, was responsible for the design of the bridge. Iskander Helmy el Gamal Bey was Inspector General, Way and Works (Bridges), which was the Department concerned. The work was under the direct control of the Head of the Bridge Department, Soad Seoudy Bey, with Ezz el Din Bey as Resident Engineer on the site.

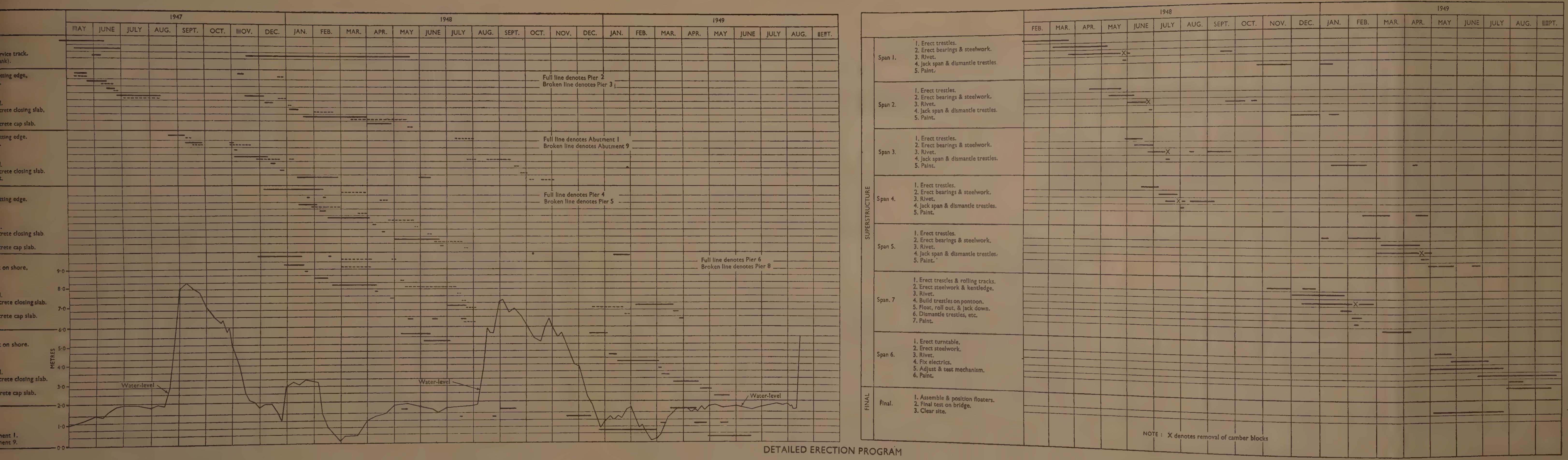
FIGS 2



GENERAL ARRANGEMENT

SUBSTRUCTURE	Preliminary	1. Establish site & unload plant. 2. Erect generators & compressor 3. Construct Low Nile jetty & lay 4. Construct High Nile jetty (East
	Piers 2 & 3	1. Erect cranes & prepare bed for 2. Unload & erect working chamb 3. Concrete haunches. 4. Sinking & concreting. 5. Concrete working chamber & s 6. Shutter, fix reinforcement, & c 7. Lay masonry & concrete pier. 8. Shutter, fix reinforcement, & c
	Abutments 1 & 9	1. Erect cranes & prepare bed for 2. Unload & erect working chamb 3. Concrete haunches. 4. Sinking & concreting. 5. Concrete working chamber & s 6. Shutter, fix reinforcement, & c 7. Lay masonry & concrete abutm
	Piers 4 & 5	1. Tip island. 2. Erect cranes & prepare bed for 3. Unload & erect working chamb 4. Concrete haunches 5. Sinking & concreting. 6. Concrete working chamber & s 7. Shutter, fix reinforcement, & c 8. Lay masonry & concrete pier. 9. Shutter, fix reinforcement, & c
	Piers 6 & 8	1. Drive staging & erect cranes. 2. Erect working chamber steelw 3. Launch & position. 4. Concrete & sink to river bed. 5. Sinking & concreting. 6. Concrete working chamber & 7. Shutter, fix reinforcement, & c 8. Lay masonry & concrete pier. 9. Shutter, fix reinforcement, & c 10. Dismantle staging.
	Pier 7	1. Drive staging & erect cranes. 2. Erect working chamber steelw 3. Launch & position. 4. Concrete & sink to river bed. 5. Sinking & concreting. 6. Concrete working chamber & 7. Shutter, fix reinforcement, & c 8. Lay masonry & concrete pier. 9. Shutter, fix reinforcement & c 10. Dismantle staging.
	Wing Walls	1. Casting piles for both abutmen 2. Driving piles for abutment 1. 3. Driving piles for abutment 9. 4. Lay masonry & concrete for ab 5. Lay masonry & concrete for ab

Figs 4



DETAILED ERECTION PROGRAM

The Contractors were Messrs Dorman, Long & Co., Ltd., whose Agent at the site was Mr W. H. Webb.

All the steel used in the work was supplied from the Contractors' mills, the fabrication of the superstructure being carried out by Messrs The Butterley Co., Ltd, of Derby, and the machinery by Messrs Head, Wrightson & Co., Ltd, of Thornaby.

Of the joint Authors, Mr K. E. Hyatt, the Chief Bridge Engineer for the Company, was responsible for general arrangements and progressing of the construction from England and Mr G. W. Morley was the Contractors' Sub-Agent and Chief Engineer on the site.

The Authors wish to thank Messrs Dorman, Long & Co., Ltd, for permission to present the Paper and Mr J. F. Pain, Director and Manager of Bridge Department, for his assistance and advice in its preparation.

The Paper is accompanied by seven photographs and eight sheets of drawings, from which the half-tone page plates, folding Plates 1 and 2, and the Figures in the text have been prepared.

Discussion

The Authors introduced the Paper with the aid of a series of lantern slides.

Mr H. Shirley Smith congratulated the Authors on the Paper and photographs, which he had found most interesting, and also on the restrained and modest way in which they had referred to the difficulties which must have been encountered. He thought that a great deal of tact and resource must have been exercised to complete the work so smoothly.

First, he would like to ask for more information about the compressed air work. There had been two 3-inch air connexions, one into the lock and one into the working chamber. (By coincidence, that was exactly the same arrangement which his own Company had just adopted, quite independently, for caissons for a job in Sydney.) The delivery to the working chamber admitted fresh air direct to the sinkers and was the one normally used; the connexion into the air lock was the stand-by supply. (In the caissons at Sydney it was proposed to sink through sand by open dredging and not to put compressed air on until rock was encountered.) He recalled that some years ago, an accident had occurred to the air shafts used on the foundations of the Bangkok Memorial Bridge, which had been attributed to the fact that they were "figure eight" shafts; they had been replaced by specially made cylindrical shafts, which had guides inside them to ensure that the bucket went down one part of the shaft; the men used a ladder in the other part. A circular section was obviously better shaped to resist air pressure than a "figure eight." He would be glad to

hear the Author's views on why the Contractors had now abandoned the cylindrical shaft and reverted to the "figure eight"; his Company had always used the "figure eight" section.

He would like to know whether the Contractors used an ejector pipe for discharging sand when "blowing down" the caisson. He had been glad to see that only $\frac{1}{4}$-inch-thick-shell plating had been used; at one time very thick steelwork was customary, but it was generally appreciated now that $\frac{1}{4}$-inch was usually ample. He noted that there had been difficulties in cooling the air; that always seemed to present a problem. Fortunately, in hot countries, it was always cooler in the working chamber than in the air locks—even though the locks were covered with matting and kept sprayed. Presumably that was because the hot air inside the shaft rose.

He would like the Authors to give the final position of the caissons—their maximum divergence, at top and bottom, from the theoretical position.

With regard to skin friction, the only figures which he had measured personally were in connexion with the New Howrah Bridge, and worked out at about 4 cwt per square foot, which seemed to agree fairly closely with the values given in the Paper.

Why had compressed air been used at all? The old bridge had been built in 1875, and presumably compressed air had been used then. For 75 years foundations for bridges on the Nile had been put down in compressed air. Every case must, of course, be considered on its merits, but the conditions for the Kafr el Zayat bridge seemed to be ideal for open dredging. He was aware that the foundations had been designed by the Egyptian State Railways and they had specified pneumatic sinking; the decision had not been left to the Contractor. The caissons had been founded on a sand bed, and could easily have been concreted under water. There were no obstructions in most of the foundations, and arrangements could have been made in advance to enable air to be put on, if and when obstructions were encountered. The sinking would have been quicker and cheaper by open dredging and it might have been possible to get the foundations down in the first season. In the Nile, there were several metres of scour during the floods; that depth of sand was shifted and re-deposited by the river every year, so that it could not matter if the contractors disturbed the river bed a little. He was strongly in favour of compressed air where it was needed, but he would welcome the Authors' views on whether it had been required for the work described in the Paper.

Mr W. H. Webb observed that comment had often been made on the amount of work involved in the preparation of Papers for the Institution, and he would like to give some evidence in that respect. As the Agent for the Contractors on the construction of the Kafr el Zayat Bridge, it had been his duty at the end of the construction to clear away the Contractors' office and their remaining effects from the site. In so doing he had had to handle the records which Mr Morley had kept of the work; six coolies

had been required to carry them, which meant that they weighed between 300 and 400 pounds. That showed the tremendous amount of work required to obtain those records and the very complete set of figures provided in the Paper. It appeared that those figures were full enough to make it possible to programme a similar work without reference to any other document.

Mr Webb referred to *Figs 6*, which illustrated the novel method developed on the site by Mr Morley and adopted for the assembly and flotation of a caisson. He said that the square tanks shown had previously been used for the support of a pile frame to drive piles for the temporary work. Subsequently they had been used for the support of a floating crane for the span erection. They were actually ex-Army pontoon units, but any tank made up of Braithwaite's pressed steel tank plates, or any other suitable material, could have been used. He stressed that that method was extremely economical because, apart from a few stiffening beams, there was no additional material in use, which would not be required on the site for other work.

He then referred to the contract price for the whole work which, as mentioned on p. 103, was £561,070. That, in his opinion, was an extremely economical price for a bridge of such a size in the post-war period. Such a price could be adhered to only if extreme economy was exercised in construction. He thought that all engineers would have to look to the economy of their works in the near future, because, although British engineers held what was probably the premier position in the world for the construction of similar works in tropical and sub-tropical countries, German and Japanese engineers were now contracting in Egypt, India, and Pakistan, and it would be necessary to offer extremely competitive prices if British engineers were to keep their position.

In the case of the Kafr el Zayat Bridge, much of the economy had been effected by the good arrangements which had been made for the despatch of the material from Britain to the site. Many engineers might have been inconvenienced, as he had been in the past, by the arrival of complete floor systems (because they were so much easier to fabricate) for a bridge before the bottom booms. At Kafr el Zayat, however, every span and every caisson had arrived complete to the last detail, despite the difficulties which the Authors had mentioned, and which presumably were common to most major works undertaken in the same period. Usually the materials arrived at such a time as to enable them to be unloaded from the barge to a position whence they could be hoisted directly up to their place in the span. That saved all storage charges, which could be a tremendous item on such a bridge. The responsibility for this had rested with Mr Hyatt, who had effected such good delivery under difficult conditions.

Mr O. A. Kerensky asked a number of questions, the first dealing with the construction of the piers. He gathered from the Paper that the

Contractors had had very little say in the design of the piers and the provision of the steel caissons, but he would like to know whether any other form of construction of the caissons had been considered. In particular, had concrete caissons been considered, and had the Contractors had the right to substitute a concrete caisson for a steel caisson, and particularly for the steel plating above the caisson? Six of the piers had been sunk on dry land, and it seemed that the use of steel in that case was hardly justified.

He observed that the shell plating had been joined by a system of riveting and welding, and wondered whether welding, with a few locating bolts at about 12-inch centres, would not have been sufficient. That procedure had been successfully adopted in the case of the caissons for the Baghdad Railway Bridge [1] but possibly it had been a matter of design about which the Contractors had no say. He would like to know how much riveting had been done, and whether it was just a stitch riveting, or were the caissons riveted and then welded?

Secondly, with regard to the sinking of the piers, had open grabbing been prohibited, or had it not been considered, or had it been considered and found to be uneconomic? Since the caissons had not been founded on rock, there would have been no need to examine the bottom, and the concrete could have been deposited under water at very much less cost and with very much less risk.

Thirdly, he said that the Paper stated that the Egyptian State Railways had restricted the pouring of concrete to the period between 6 a.m. and 10 p.m., and had insisted on the inner shutters being struck not earlier than after 48 hours. That would undoubtedly interfere very seriously with the work of the Contractors, and it would be interesting to know why such a restriction had been imposed. Had any fault been found during the night shift, or had there been any risk involved in lack of supervision? It seemed to him an interference with the Contractors' freedom with a process which should be continuous to be efficient.

Fourthly, he referred to what he thought was a remarkable incident. A caisson had been sealed and left flooded for 3 months, and sinking had then been re-started without difficulty. That was the first time he had heard of such a thing being done, since normally, sinking a caisson, one would imagine, was a very continuous process and also a very delicate one. In general, one could not but admire the confident familiarity with which the caissons had been handled—water, concrete, and air being taken off and on at will—as if the caissons had been toys in a bath. It showed complete confidence in the methods adopted.

Fifthly, regarding skin friction, he said that Messrs Reid and Sully, in their Paper [2] had made the following statements: "(1) None of the

[1] A. E. Reid, and F. W. Sully, "The Design and Construction of the Baghdad Railway and Road Bridge." J. Instn Civ. Engrs, vol. 36, p. 429 (Nov. 1951).

[2] "The Construction of the King Feisal Bridge and the King Ghazi Bridge, Baghdad." Works Construction Paper No. 4, Instn Civ. Engrs, 1946.

Authors' measurements has ever shown a value of skin friction higher than 4 cwt per square foot in any material. This value is that established during continuous sinking operations. (2) Any apparent values higher than 4 cwt per square foot have always been traced to distortion of the caisson in construction. A very small irregularity in the skin of the caisson, a small bulge in the plating, or a departure from the parallel in the sides, will have a disproportionate effect upon the measured value of skin friction." Messrs Hyatt and Morley had stated almost exactly the opposite. They had quoted skin frictions of more than 4 cwt per square foot, and had stated that "there is no evidence that the smoothness and regularity of the surface had any beneficial effect on the ease of sinking in sand and silt." The Baghdad caissons had also been sunk in sand and silt, and it seemed to him that further expert evidence was required before the decision to incur the considerable expense of providing smooth walls in cassions was taken.

Sixthly, he was of the opinion that the superstructure appeared to be ideal for cantilever erection, and indeed the provision of dummy panels at the piers tended to confirm that suspicion. Had that been considered and found to be uneconomic ? In the light of experience with floods, would the Authors again adopt erection on staging, rather than cantilevering ?

Lastly, he said that the information on painting was of great value, but could the Authors give some details of the cleaning ? Had all the scale been removed prior to painting in the workshops and, if so, by what method ? If that had not been done, it would be interesting to know if the paintwork was still satisfactory. Further, had the Egyptian State Railways adopted special precautions to avoid unpaintable surfaces, for example, by planing all web plates flush with flange angles ?

The Paper stated that timber sleepers had been secured direct to the top flanges of the stringers, and he thought that that would produce corrosion trouble in the future.

Mr T. H. Hopkins recalled the story of the War Department, in the 1914–18 war, sending out concreting materials, including sand and ballast, to Egypt. That had seemed strange until he had discovered how often it was extraordinarily hard to find any sand or ballast near a site in Egypt which was suitable for building purposes. It was possible to get plenty of sand to mix with lime for common building, but it was difficult to obtain the right material for engineering purposes. The Authors had stated that considerable difficulties had been experienced in maintaining the specification for sand and ballast, and it would be interesting to know where the required material had been obtained.

There were nine big bridges over the Nile. Of these, the Kafr el Zayat Bridge had been built by an Italian firm, and most of the others by Belgian firms, but two had been built by British firms. One of those was at Desouk, built in 1926 by Messrs Dorman, Long, and the other had been built by the Cleveland Bridge Company at Edfina. Had it been necessary

to make many modifications in the design of the bridge, of the steelwork and other work, as it had been put to them? Mr Hopkins said that he had been Bridge Engineer for the Egyptian State Railways for some years, and that the bridge at Edfina had been designed in his office. The Cleveland Bridge Company had asked for a great many modifications, but although they would have made erection simpler, they would also have increased the weight considerably, and the Egyptian State Railways had not felt inclined to bear the additional cost.

Steel castings to carry the rotating turntable for the same bridge had been unobtainable in Britain and he would like to know where those castings for the new Kafr el Zayat bridge had been made.

Reference was made in the Paper to an 8-hour shift and to a maximum pressure of 38 pounds per square inch. Had that shift of 8 hours been maintained with that pressure in operation?

Referring to the different religions of the workers employed, he said that in Egypt there was such a mixture that in the diary issued by the Egyptian State Railways, the almanac contained three distinct calenders —Moslem, Jewish, and Christian—and showed the dates of the feasts and fasts of the various denominations; different time off was allowed all the year round to meet their separate requirements.

Mr J. P. M. Pannell referring to the re-use of the military equipment, said that he had found, as no doubt the Contractors for the Kafr el Zayat Bridge had also found, that the P.C. tank was one of the most useful pieces of equipment evolved by the Royal Engineers in the last war. It would be interesting to know whether the Contractors had acquired their tanks from local dumps (in which case they would probably have been already assembled), or from R.E. stores and had had to assemble them. If it had been the latter case, had local labour been used, and had any difficulty been experienced with the erection and with the sealing?

Mr L. E. Hawkins observed that the steelwork had been designed by the Egyptian State Railways, and that the Authors had said that it was for the standard loading, but it would be interesting to know what impact factor had been taken on that loading for hammer-blow effect and any other dynamic effects.

He agreed with Mr. Kerensky about the dummy panels. So far as it was possible to judge from the photographs, they detracted from the appearance of the bridge. Would the Authors say whether they thought that the bridge would have looked better or not so good without those dummy panels over the piers?

In the Paper there was a reference to the military trestling as "V" trestling, but the photographs had led him to think that it was military "L" trestling.

In the cross-section of the bridge the rails were set-in about 5 inches from the stringers. Would the Authors say whether there was any particular reason for that? It was not easy to see from the sketch, but he gathered

that there was no deck over the whole bridge, in the four-foot way or outside, and therefore the cross-timbers were beams in regard to that eccentricity. He would like to know how thick the cross-timbers were and whether they were put specially close together to carry a de-railed wheel, or whether there was any special device to protect the bridge in the event of the passage of a waggon with one axle derailed.

Mr K. C. Burden congratulated the Authors on the precise and methodical manner in which they had recorded every conceivable detail. He agreed with Mr Shirley Smith that it was surprising that in Egypt the practice had invariably been to sink foundations in the Nile with compressed air, although the conditions would appear to be ideal for sinking by open grabbing. He suspected that the reason had been a fear of running into trouble when sinking in open wells. When one sank a foundation in the open, one always wondered what could be done if it proved impossible to get the well down below the level to which it had to go on account of scour and stability. He had been faced with that problem on one occasion with a well in Burma. He had estimated the skin friction at about 3 cwt per square foot, and, because of the inaccessibility and other reasons, he knew very well that he could not have got a load on to that well to sink it if the skin friction had proved to be more than 3 cwt per square foot. To provide some sort of lubrication down the sides, he had made arrangements to build in jet pipes which would discharge water near the cutting edge, and also to introduce jets round the outside edge, but the well had been sunk 120 feet and founded without having recourse to any of those measures, so that presumably the friction had not exceeded 3 cwt per square foot.

On the question of painting, he said that on a bridge in a country such as Egypt, the site staff were always harried by the Government inspectors on their painting, and for the very smallest thing they were stopped and delayed. He had always wondered whether the steel had been carefully prepared before the red lead coat had been put on. He had seen great sheets of red lead paint peel off bridges of which the components had been 6 or 7 months in transit from the works to the site, and that had been entirely attributable to the paint having been put on top of the scale. He suspected that the inspector in the shop did not take nearly so much trouble as the inspector on the site, yet that first coat of paint was far more important than the subsequent coats.

The Authors, in reply, referred to the air connexions and the idea of putting one pipe in the roof of the working chamber and one going to the air lock. The Authors' firm had done that for a very long time; moreover, they had both pipes open at the same time, and it was not simply an insurance that if something went wrong with one there was another which could function. There were the usual non-return valves on the lock, so that if the air-supply was interrupted it did not let the caisson down. The "figure eight" shaft had been used instead of a circular shaft owing to

greater availability. There was, however, a difficulty about the use of circular shafts, namely that the space which the men were supposed to use to go up and down, so as to keep out of the way of the bucket, was too small for a man comfortably to get into on the ordinary 5-foot shaft which had been evolved after the accident to which Mr Shirley Smith had referred. There was also the difficulty of adapting a circular shaft to " figure eight " locks, and only " figure eight " locks had been available.

So far as the ejectors were concerned, the contractors had had some bright ideas; they had not intended to use them as ordinary air ejectors, but had hoped to pump the sand out. However, for lack of sufficiently powerful pumps the experiment had been a failure, and they carried on with the ordinary buckets. Buckets were an extraordinarily efficient means of getting material out of a caisson. There had been no difficulty in keeping pace with the job, and normally only one 8-hour shift in the 24 hours had been required to keep the caisson moving and keep pace with the concreting above.

The Authors thought that the temperature had been better down in the working chamber (mentioned by Mr Shirley Smith) largely because the working chamber was in intimate contact with the ground, 50 to 80 feet down, where it was very much cooler than on the surface.

Those who had worked in Egypt would know that normally all Egyptians did not take the fast of Ramadan quite so seriously as it was apparently taken in India. Mr Hyatt remembered a squad on one job, which Mr Hopkins might also recollect, where a night shift was worked right through Ramadan, something which was almost unheard of in normal circumstances. Ramadan happened to fall that year just before the flood, so that there had been no possibility of ceasing operations for that reason.

Several speakers had asked why compressed air had been used and not some other method. The short answer was that it had been specified by the Egyptian State Railways. It was not for the Author to say why the Egyptian State Railways, and indeed the Roads Department and all the other authorities there, always specified compressed air. They thought it was because it was the method which had been approved and well tried. They believed that the method of sinking by open wells would be entirely satisfactory so long as precautions were taken for the use of air in special circumstances, such as had occurred in the case of pier No. 8 of the Kafr el Zayat Bridge, where there had been a large quantity of stone and where the open well would not have worked very well.

The question of using piles for the bridge foundations had not been mentioned, and the Author thought that that was something which would be worth investigation in Egypt. Mr Hyatt had designed several small bridges many years ago using ordinary pre-cast piles. They carried the load on the silt entirely by friction. There had been occasions when a pile had had almost to be held up to prevent it slipping out of sight, but

had carried a 30-ton test load quite satisfactorily later. A great deal of work had been done since then on very long piles, 30 metres (98½ feet) long, or of that order, in other countries—particularly Scandinavia. Mr Hyatt did not see why they should not be used in Egypt, and for one job for which his company tendered they proposed using rather long circular piles, about 24 inches in diameter. There had been no opportunity of proving whether such piles would be successful or not.

There was another point which might also be in the minds of the Egyptian authorities. They did not like concrete placed under water, and the Authors thought that they might have had some rather sad experiences in the past with unsatisfactory methods. That might have had a good deal to do with their reluctance to use anything other than the pneumatic caisson method.

Mr Webb had been the firm's Agent on the site for the greater part of the time that the work on the bridge was going on, and undoubtedly deserved a great deal of the credit for the good work done on the site. The Authors appreciated his remarks about the way in which the material came forward, because, looking back, it seemed little short of miraculous. It had not been Mr Hyatt's entire responsibility, but was the result of very strenuous efforts by everybody in the head office of his firm.

What the Authors had said about wells covered some of Mr Kerensky's questions. The use of concrete caissons without steel shell plating had been considered, and was turned down by the Egyptian State Railways. For most of the work it would probably not have been economical, because the construction of the caissons would have taken very much longer. There had been restrictions on concreting, and the reason for the restricted hours was that the Egyptian State Railways had been unable to provide inspectors for the job. Concessions could not be obtained on that point, except for the two caissons in the first rushed season. It had been a continual source of annoyance. It had been intended to sink the two abutment caissons without any steel shell plating, but the contractors had finally been ordered to produce steel shell plating for them. It had suited those on site, because it had been possible to do a quicker job, but it had cost a good deal of money.

With regard to the combination of riveting and welding, the rivets were not at the usual close pitch; they were spaced about 6 inches apart. The welding on the ¼-inch fillet, which was all that could be got in, was not, from calculations, considered sufficiently strong in certain circumstances. The method certainly provided a convenient means of getting a watertight job.

Mr Kerensky referred to the apparent confidence shown in sealing up caissons at the end of one dry season and restarting them at the beginning of the next. The Authors did not know how much of that apparent confidence was real; they had had no option but to do it, and had been very relieved to find that it worked. On the question of the difference between

the present Authors and their friends Messrs Sully and Reid, the Authors could only say that in the Paper they had given the facts as they had found them, and two caissons had had the plates badly distorted; it was not until the later stages that the local manufacturers produced anything approximately plumb and square.

Cantilever erection had of course been considered, but on the figures available at the time it would have been definitely more expensive than supporting the spans on falsework. The Authors thought that the redundant members at the ends of the spans had been put in only for the sake of appearance. There would be a difference of opinion on whether they did improve the look of the bridge or not, but they were far too light to be of any use for cantilever erection, and the conclusion was reached that the method actually adopted would be more economical. Where there was a site on which economical falsework could be produced, the Author believed that that was the best and cheapest way to do the work, though it did not give one so much scope for being ingenious. One could get the job together more quickly and save money on drifts, which had been very expensive in recent times. Mr Hyatt did not recommend cantilever erection unless the crossing was such that falsework could not be put in reasonably cheaply. He had not always held that view.

Words without end had been written about painting. In the case in question, the specification merely said that it had to be cleaned with scrapers and wire brushes, and one knew what that meant; it was not possible to remove all mill scale, or even rust, with that method, even with the most careful inspection. In reply to the inquiry whether the paint had stood up, the Authors could say that it had not. It had not deteriorated seriously, but it required touching up to the extent of about 5 per cent after 2 years. They believed that most employing engineers were disinclined to re-paint a bridge soon enough after it had been first done by the Contractors. It was not possible to get mill scale off without sand blasting or some similar process, and, if the scale was not taken off earlier, it came off later and brought the paint with it.

The Authors were very interested in, and very well aware of, the newer processes which were now being tried. They had used sandblasting in the past at Storstrøm in Denmark, and were now doing the same in Portugal. In his opinion sand blasting or some other method to get the scale off was desirable. It did not matter so much what was put on, whether metal spray, red lead, or other treatment, so long as it was put on when the steel was dry and as soon as possible after it had been cleaned.

The question of voids and cavities in steelwork was exercising the minds of many people at the present time. The Authors did not think that any particular precautions had been taken in that respect on the job in question, but bridges in Egypt did not suffer badly from corrosion, because there was not a great deal of rain in winter in Lower Egypt, and

there was not normally much condensation. The bridges which had been taken down there in the past had always been in fairly good condition.

The laying of sleepers direct on the stringers was standard Egyptian State Railways practice, and probably Mr Hopkins could confirm that it had been their standard practice for a great many years. It had certainly been so for 30 years, as long as Mr Hyatt had known it. It seemed to work perfectly well. The sleepers were about 5 inches thick, laid direct on the stringers, and the extended flanges took two bolts through each sleeper.

Concrete materials, to which Mr Hopkins referred, were always a very vexed question. The Contractor always thought that the Engineer was being too fussy, and the Engineer thought that the Contractor was trying to get away with something. It was very difficult to get suitable sand from any one source, but it was possible to get it by taking it from different sources and by going to a great deal of trouble in grading it. The results of test cubes made with natural run sand and suitable crushed or screened ballast, however, were usually good enough for normal conditions. There were too many fines in most of the sand. That was the trouble which one came across in many countries, and Mr Hyatt had struck it recently in places as far away as Borneo and New Zealand.

No modifications had been made in the design. The contractors had had a similar experience to that of the Cleveland Bridge Company when building the Desouk Bridge, where the extra steel had not been paid for. The contract made the Contractor responsible for the adequacy of the design, and that was, naturally, checked.

The steel castings for the turntable for the bottom roller path had been made in Great Britain by Messrs Head Wrightsons of Thornaby. There had been certain difficulties in the early stages but they had been overcome. The Authors did not think that the standard Egyptian State Railways design was very helpful; it was difficult to produce a good casting to the shape and section required.

In reply to Mr Hopkins's last question, 8-hour shifts had been worked at the full pressure of 38 lb. per square inch and there had been no trouble of any sort.

Mr Hyatt would like Mr Pannell to tell him where he could get some more P.C. pontoon tanks, because he agreed that they were one of the most useful things that had ever been developed. They had been bought in the fully-erected condition, but had been knocked down for transport abroad; there had been no difficulty in putting them together again. The contractors had used them in two or three different countries in widely different parts of the world, with very satisfactory results.

In reply to Mr Hawkins, the Impact Factor was $\frac{36}{27 + L} \times 100$ per cent, where L denoted the loaded length in metres. For the various conditions,

in the case of the fixed span, the factors applicable to different parts of the structure were as follows :—

Stringers :	106 per cent
Intermediate cross-girders :	65·4 per cent
End cross-girders :	86·5 per cent
Chords of trusses :	21·6 per cent
Wet members :	21·6—65 per cent

The military trestles were a mixture of "V" and "L" trestles.

The Authors had already referred to the question of stringer flanges and the carrying of the rails direct on them. The sleepers had been closely spaced—about 60 centimetres ($23\frac{1}{2}$ inches). There were no special arrangements about derailment, except that there were check rails right across the bridge. The only place where it did not look as if they would operate very well was at the special joints at the junctions of the swing span, but no serious difficulties had occurred there.

Turning to Mr Burden's remarks regarding painting, Mr Hyatt mentioned that on a job with which he had been intimately connected—the Menai Bridge—the main links had been carefully sand blasted, metal sprayed, and painted with red lead, and were showing no signs of corrosion after 10 years or more, whereas the rest of the steel portions of the bridge, which had been just normally cleaned and painted, gave very serious signs of corrosion within about 18 months after the work had been completed.

With regard to the sealing of the caisson at the end of the first season so as to re-start it in the second season, the Contractors had had no confidence at all that it would remain in its position. No information had been available about the previous sealing of caissons, so that they had had to make it up as they thought fit on the site. On a future occasion, the Authors would not recommend putting the concrete seal on the bottom. Possibly it was very useful, but it had been a great nuisance at the beginning of the second season.

The measurement of skin friction had been very difficult, because when a caisson had sunk normally there was always sand or some other obstruction in contact with the cutting edge, so that it was difficult to say when the caisson was held entirely by skin friction. Maybe that was the reason for the large discrepancies in the values given by different engineers. The only two good measurements were the one mentioned in the Paper for caisson No. 3 when it was in a good position, where the skin friction was 4·8 cwt, and the one for caisson No. 7 in its final position when it was cleared out for concreting. Caisson No. 3 was the one which had been most distorted, and why it should have had the smallest skin friction it was very difficult to say.

PUBLIC HEALTH ENGINEERING DIVISION MEETING

1 January, 1952

HENRY FRANCIS CRONIN, C.B.E., M.C., B.Sc., Vice-President I.C.E., Chairman of the Division, in the Chair

The following Paper was presented for discussion and, on the motion of the Chairman, the thanks of the Division were accorded to the Authors.

Public Health Paper No. 4

"Some Problems in the Disposal of Industrial Effluents and Domestic Wastes"

by

John Thornton Calvert, M.A.(Oxon.), M.I.C.E., F.R.I.C.,

and

Peter Maurice Amcotts, A.M.I.C.E., A.M.I.W.E.

SYNOPSIS

Towards the middle of the nineteenth century, it was discovered that enteric diseases could be conveyed in drinking water and that sewage often harboured these diseases. Since then civil engineers, with the co-operation of the medical profession, chemists, and others, have been striving to eliminate these water-borne diseases by engineering means.

The Paper briefly surveys the progress made in the task of satisfying the ever-increasing demand for safe water-supplies, and in the purification and disposal of industrial and domestic effluents, referring to the work of the Royal Commission on Sewage Disposal and the Water Pollution Research Board of the Department of Scientific and Industrial Research and discussing the Rivers (Prevention of Pollution) Act, 1951.

The main part of the Paper describes in detail some recent examples from the Authors' experience of the methods employed in the treatment and disposal of effluents from various sources and discusses the design of the engineering works involved.

In conclusion, the Paper outlines some of the problems now facing the Public Health Engineer caused by the continually increasing demand for water-supplies, the consequent need for improved standards of effluents, and the complexity of new industrial processes.

INTRODUCTION

IN 1896, Joseph Lecky, in his study of "Democracy and Liberty," wrote that the great work of sanitary reform had been perhaps the noblest achievement of his age. Is this not just as true to-day ?

The lay population tries to pretend that such problems do not exist and tends to despise those by whose efforts cleanliness is maintained. The

Institution, however, by inaugurating the Public Health Engineering Division, has opened the way to an even better understanding between the medical and engineering professions and in so doing will further the cause of greater knowledge of the scientific problems involved in a most important subject. The progress of our industrial, economic, and social welfare depends irrefutably on the maintenance of a healthy population, and it is imperative that the utmost vigilance be required of all those responsible for the provision of safe water-supplies and the disposal of effluents, of whatever nature.

Apart from a brief historical survey of past progress and some comments on problems as yet unsolved, the scope of this Paper is confined to some recent examples within the Authors' experience of methods used in the treatment and disposal of industrial and domestic effluents from various sources, with some details of the biochemical and design problems involved in individual cases.

Historical

Much has been said recently about the improved standard of health and the reduction in the death rate during the past 150 years by the efforts of the engineer and others to improve the supply of water and the disposal of it after use.

Perhaps at the outset it would not be amiss to refer to the Presidential Address given in 1947 by Sir Roger Hetherington,[1] who so ably reviewed the developments in water-supply and drainage from the early nineteenth century to the present day. Sir Roger Hetherington discussed the dangerous conditions created as a result of urbanization caused by the Industrial Revolution, and he pointed out that it was the engineer who had to tackle the problems of increasing the supply of water and of securing it against pollution. Dr Beattie [2] has given a valuable illustration of the co-operation amongst those concerned in achieving the improvements he surveyed.

As the pace of the Industrial Revolution quickened, the influx of rural inhabitants to the newly formed urban areas created problems of sanitation that were for many years almost entirely ignored, other than by a few far-seeing and courageous men, notably Edwin Chadwick, who against great opposition worked for sanitary legislation and reform. Dr Beattie referred to the overcrowded and unhealthy conditions of the nineteenth century and it is proposed in this Paper to survey only briefly the events that lead up to the conditions prevailing at the present day.

In the middle of the nineteenth century, when steps were taken to ensure the discharge of sewage into drains, the rivers and waterways to which the sewage was conveyed became nothing less than open sewers. When it is recalled that much of the water-supply of the community,

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[1] J. Instn Civ. Engrs, vol. 29, p. 1 (November 1947).

[2] N. R. Beattie, "Civil Engineering and Medicine in the Field of Environmental Health," Public Health Paper No. 2, Instn Civ. Engrs, 1951.

particularly in the London area, was obtained from these same rivers in its raw state, it is not surprising that the death rate and incidence of water-borne diseases remained distressingly high until the turn of the century.

The work of the bacteriologist and others gradually supplied the scientific information that led to the Public Health Act 1875 and the Rivers Pollution Act 1876. These Acts were introduced to prohibit the discharge of raw sewage into inland waterways and some form of treatment, therefore, became necessary.

The first form of treatment consisted of land irrigation which continued until many thousands of acres were sewage-sick and useless. Intermittent dosing of land of high permeability gave good results and through many stages of evolution this, in turn, brings the narration basically to the percolating filters of the present day. The Rivers Pollution Act of 1876 saw the beginning of control of the standard of effluents from domestic and industrial sources permitted to be discharged into watercourses, and the Royal Commission on Sewage Disposal (1898 to 1915) for the first time recommended the adoption of legal standards of purity of the sewage effluent, so that the discharge into a stream should be an offence under the Act of 1876 only if the prescribed standard adopted was not maintained.

The Recommendation of the Royal Commission, based on the assumption of at least eight times dilution by the recipient watercourse, was that an effluent, to be satisfactory, should not contain more than 3 parts per 100,000 of suspended solids, and should not take up more than 2 parts per 100,000 of dissolved oxygen in 5 days at a temperature of 65° F. The Commission further recommended that higher or lower standards may be prescribed as local circumstances require or permit, but the Commission laid down no precise standard for variations from the normal as they considered that these cases must each be treated on their merits.

The Recommendations will be familiar to those concerned in these matters, but it may not be generally realized that the proposals of the Royal Commission were never given Statutory Authority and, until 1951, the powers for preventing pollution were still those of the 1876 Act.

The Rivers (Prevention of Pollution) Act, 1951, goes a long way towards meeting the recommendations of the Royal Commission, and although it does not prescribe a general standard applicable throughout the whole country, it does enable standards to be established by the River Boards for the watersheds under their jurisdiction; and since these standards are subject to the approval of the appropriate Ministry there should be a measure of uniformity throughout the country.

No doubt many difficulties will arise and many decisions will have to be taken before the effects of this legislation are observed, but eventually it will undoubtedly be more satisfactory to have prescribed standards rather than the previous indefinite position where the requirements are stated in such general terms as to provide scope of unending argument.

The development of innumerable industrial processes, particularly

during the past 60 years, has produced trade effluents that have created complex problems for the scientist in his efforts to purify the rivers and safeguard water-supplies. Much praise is due to the Water Pollution Research Board of the Department of Scientific and Industrial Research, whose work in this field has resulted in an ever-increasing knowledge of the effect of material in solution and suspension discharged into rivers and streams and of the methods of treating effluents of both industrial and domestic origin.

Industrial Effluents and the 1937 Act

Industrial effluents can be of a very polluting character and the Rivers Pollution Act of 1876 provided only that the best reasonable and practicable means should be employed for their purification; that frequently resulted in harmful effluents being discharged into the streams of Great Britain.

In many cases the best means of treating industrial effluents is by admixture with domestic sewage, but until 1937 an industrialist had no right to discharge his effluent into the sewers of a local authority, and was forced to adopt independent treatment in spite of the fact that he may have been unable to produce economically an effluent as satisfactory as would be possible at the authority's sewage treatment works.

In this respect, local authorities in the past have adopted very different attitudes; some have maintained the view that the reception of trade effluents, often without charge, is desirable in order to attract trade to their districts; others have consistently refused to accept such wastes on the grounds that the cost of treatment should be a burden on the particular trader concerned.

The Public Health (Drainage of Trade Premises) Act of 1937 to some extent reconciled these opposing views in that it gave the manufacturer the right to discharge his effluent into the public sewers, but at the same time it provided that he should make a proper payment for its reception and treatment.

In the Authors' opinion the great majority of industrial effluents can be most economically treated by the local authority with the sewage of the town, but it is important to realize that there are exceptions to this generalization and the Act gives the authority the power (subject to appeal) to refuse to accept trade effluents in these cases.

The authority can, in addition to demanding payment, stipulate conditions under which they are prepared to accept trade wastes and by doing so can overcome many of the objections to the acceptance of the effluents. In particular there are some processes which result in toxic products which would inhibit biological purification of the sewage; the presence of large quantities of solids in suspension would tend to choke sewers and there are corrosive substances which can attack the fabric of the sewerage system.

All these difficulties can be overcome by pre-treatment at the manufacturer's works but by limiting the pre-treatment to the essential minimum, the greatest economy is generally achieved.

The Act provides for the making of by-laws applying to all trade wastes discharged into the sewers of the town, but experience has shown that it is usually preferable to employ the section of the Act permitting the formulation of agreements for the individual case, whereby co-operation between the factory and the local authority is assured in the interests of both parties.

One of the most criticized sections of the Drainage of Trade Premises Act is that giving prescriptive rights to traders for effluents discharged into sewers before the passing of the Act and in the operation of this section the new towns being established under the New Towns Act of 1946 are in a more favourable position than many of the older communities.

Crawley New Town

The establishment of new towns in the London area has created particular problems owing to the fact that there is not the over-abundant water-supply of the west; and for the same reason the rivers on which the new towns are located are relatively small and unsuitable for the reception of large volumes of industrial effluents.

The Crawley Development Corporation has therefore exercised commendably careful control over the industries admitted to the town to ensure that the demand for water does not exceed the available supply and that the effluents discharged are not harmful either to the treatment of the sewage or the purity of the watercourses. Industrial sites or standard factories are leased only by the Corporation and, before granting leases, careful investigations are made as to the water requirements and the type of effluent the applicant proposes to discharge to the sewerage system. Consequently, although there will be industrial wastes, it is not anticipated that their presence will cause any unusual difficulties in the treatment of the sewage. On the other hand the design of the works for the New Town has provided an interesting problem partly because of the small size of the stream into which the effluent is discharged and partly because of the financial necessity not to incur expenditure ahead of requirements. It has also provided an interesting opportunity of working on a virgin site, which is rare though by no means unique in Britain where so much of the work of the public health engineer is concerned with the extension or modernization of existing works.

Crawley has been drained on the separate system to treatment works, just outside the northern boundary of the designated area, which have been designed to deal with domestic and industrial effluents from an estimated ultimate population of 50,000 to 60,000 persons, with a peak rate of flow of four times 45 gallons per head per day of domestic sewage and twice 20

gallons per head per day of trade waste. For economic reasons the works are to be constructed in stages, each stage to deal with approximately 4 cusecs at peak flow and capable of being carried out as the population expands without wasting capital at any one stage (see Fig. 1, Plate 1).

In addition to mechanical screening and detritus-removal, the treatment works for 50,000 persons will consist of: five mechanically scraped circular primary settlement-tanks 8 feet deep, each having a capacity of 26,000 cubic feet (see *Fig. 2*); diffused-air aeration tanks, 120 feet long and 12 feet 6 inches deep, in five units of 52,000 cubic feet each (see *Fig. 4*); five mechanically scraped circular secondary settlement-tanks, 8 feet deep, each having a capacity of 26,000 cubic feet (see *Fig. 3*); three heated sludge-digestion tanks, each having a capacity of 33,334 cubic feet and 8,300 square yards of sludge-drying beds. The primary and secondary settlement tanks are designed for 6 hours' dry-weather flow, the aeration tanks for 12 hours' dry-weather flow, the digestion tanks for 2 cubic feet per head per day, and the drying beds on the basis of 1 square yard for every six persons.

The works include a pumping station (see *Fig. 5*), which will lift the sewage to the head of the works and which also houses the mechanical screens and macerators for the screenings.

Also shown in *Fig. 5* is the power house under construction, which has been built to accommodate engines driven by sludge gas and generators for the supply of electricity to drive the compressors for the aeration of the sewage. In the first stage of development with a design population of only 10,000 it was felt that the collection of gas for power generation would not prove economical and at the present time the power house contains compressors driven from the grid. Digestion tanks with gas collectors are to be installed as soon as extensions to the works are carried out.

This first stage of the Crawley works, of which a panoramic view is shown in *Fig. 6* has recently been put into commission but is as yet dealing with a load well below its designed capacity.

Dilution

Reference has already been made to the assumption of the Royal Commission that, in general, a sewage effluent would be diluted eight times when discharged into a stream, and also to the fact that this degree of dilution is not available at Crawley. The Thames Conservancy Board consequently demanded an effluent of superior quality to the general standard of the Commission and suggested that the biological oxygen demand should not exceed 10 parts per million and the suspended solids should not exceed 20 parts per million. Whilst the necessity for a high quality effluent was admitted, it was decided, after consultation with the

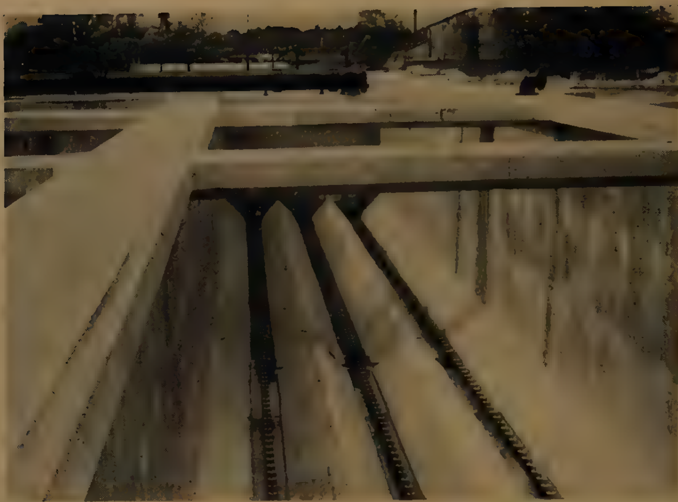
Fig 2

Inlet and Sludge Outlet to Primary Settlement Tank. Sewage Treatment Works. Crawley Development Corporation

Fig. 3

Inlet and Sludge Draw-off to Secondary Settlement Tank. Sewage Treatment Works. Crawley Development Corporation

Fig. 4



Aeration Tanks. Sewage Treatment Works. Crawley Development Corporation

Fig. 5

Power House and Main Pumping Station under Construction. Sewage Treatment Works. Crawley Development Corporation

Ministry of Health, that the works should be constructed in the conventional manner but with an excess aeration capacity, and experience would decide whether any form of subsequent treatment was essential for the protection of the Gatwick Stream into which the effluent is discharged.

The increased use of water in recent years has not only increased sewage effluent discharges but has also tended to reduce river flows in areas where the water is obtained from underground sources, and therefore the problem of adequate dilution for the final effluent from treatment works and the demand for better quality in the discharge to watercourses has become more acute in many places. An outstanding example of this situation has occurred at Luton, where the River Lee has completely dried up and the sewage effluent from the town forms the whole of the flow in the river bed. As a result of legal action, the Luton Corporation have constructed additional works consisting of rapid sand filters, designed in accordance with normal waterworks practice, and micro-strainers which also have previously been used in water-supply treatment. These processes are each treating a portion of the effluent from the original works and are producing very-high-quality effluents, having a B.O.D. and suspended-solids content of the order of 5 parts per million ; extensions, capable of dealing with the whole of the sewage flow to the treatment works, are contemplated as soon as a decision can be reached as to which process is the more economical.

Gas Liquor

The disposal of gas liquor is a perennial problem which has led to continuous controversy between the gas industry and the sewage-treatment authorities. Gasworks liquor is a very noxious liquid containing phenols and higher tar acids, together with thiocyanates and thiosulphates, and whilst phenols can be oxidized fairly easily by mature percolating filters, provided that the load is not excessive, there is invariably an increase in the B.O.D. of the effluent when gas liquor is added to the influent.

Although it would appear, from the large amount of research work which has been carried out, that the proper method of disposal is to the sewage works, there are two particular problems—one of quantity and the other of quality—where care must be exercised before accepting this general proposition.

If the area supplied with gas by the gasworks is the same as the area draining to the sewage works, the quantity of gas liquor should not exceed 0·2 to 0·5 per cent of the sewage flow depending on the methods of gas manufacture, and this proportion can be treated without unreasonable extensions to the works and without serious effects on the quality of the effluent. This condition obtained when gas was made for each town in the local gasworks, but the recent tendency towards regionalization has resulted in the concentration of larger volumes of gas liquor in towns where a central plant is installed.

The Authors were recently concerned with this problem at Farnham in Surrey when the Gas Board requested permission to discharge to the public sewers quantities of gas liquor in excess of one per cent of the sewage flow, although with a much smaller proportion there were indications of interference with biological purification as shown by the presence of thiocyanates in the effluent. The situation was aggravated in this case by the fact that the sewage works were seriously over-loaded owing to growth of the town and the impossibility of extending the works during the war. These works are now being enlarged but, until the additions are in operation, it has been considered wise not to allow an increase in the quantity of gas liquor discharged to the sewers, in spite of the obvious and unfortunate difficulty which this will cause at the gasworks.

Whenever gas liquor is discharged to sewers it is desirable that it should be measured and the flow balanced throughout the day, and that the liquor should be "spent"—that is to say, it should have the bulk of the ammonia removed—but at very small works distillation of the liquor for this purpose is quite uneconomical. Consequently arrangements have been made at Haverhill in Suffolk to allow crude liquor to be discharged to the sewers under controlled conditions to determine to what extent this will affect the effluent from a works in which settlement tanks and percolating filters are followed by an appreciable area of land irrigation.

Dairy Wastes

Another effluent that causes a considerable amount of trouble—more so recently with the establishment of larger factories—is the trade waste from dairying, milk processing, and the manufacture of allied products.

The wastes from industrial processing consist mainly of water containing milk and whey, fats, and other substances derived from milk; although not toxic in themselves, these substances undergo rapid decomposition and, on discharge to a river or stream, take up dissolved oxygen from the water, thereby reducing the oxygen content—often below the value desirable for a healthy river.

The Water Pollution Research Board of the Department of Scientific and Industrial Research has carried out a considerable number of experiments in the purification of dairy and milk product wastes and has established the possibility of treating these wastes by various processes; the Authors have been responsible for designing, and putting into operation, works using two of these methods.

An example of excessive zeal in the protection of a river occurred a few years ago when a milk products factory was erected at Coleraine in Northern Ireland. It was proposed to construct full treatment works for the effluent from the factory. Taking into consideration the trade waste discharge from a maximum future expansion at the factory, together with the domestic sewage from the town of Coleraine, it was considered that

Fig. 6

Stage 1. Nearing Completion. Sewage Treatment Works, Crawley Development Corporation

Fig. 7

TREATMENT WORKS. LEVER BROTHERS RESEARCH ESTABLISHMENT, SHARNBROOK

Fig. 8

FILTERS, CHANGE OVER CHAMBER AND HUMUS TANKS, HOLDING-UP TANK AND PUMPING STATION. TREATMENT WORKS AT SHARNBROOK

adequate dilution of approximately 1 in 500 would be available in the River Bann under minimum flow conditions, to enable effluents from both sources to be discharged in their crude form without deleterious effect on the valuable fisheries of the river. No works were constructed and, although land was set aside for this purpose should future conditions make the installation of purification plant desirable, so far no damage has been done and a considerable amount of expenditure has been saved.

In contrast with the previous case, some urgent action became necessary to render the effluent from a milk depot, in Northern Ireland, harmless to the interests of a bleaching and dyeing works downstream of the milk factory, and the method adopted for treating those milk wastes was by precipitation with a dose of " Aluminoferric " of approximately 40 parts per 100,000. The use of chemical precipitants to treat this type of effluent does not usually produce a very high degree of purification, but in this case it was not required to do so in view of the adequate dilution available.

The works installed at Dromona consist of a balancing tank, a pumping station, a mixing and dosing gear, and an upward-flow settlement tank with suspended channels to act as weirs and sludge-drying beds.

The disadvantage of this method of treatment is the necessity for skilled technical supervision and, therefore, whilst the plant at Dromona will produce the standard of final effluent required, difficulties have been experienced owing to lack of control in the addition of chemicals.

The second process which the Authors have found very satisfactory is that of alternating double filtration, in which percolating filters are operated in series, with provision for alternation of the order of treatment. This has been used successfully to deal with a flow of 15,000 gallons per day of waste from a milk-drying depot at Magheralin in Northern Ireland.

Unlike the system installed at Dromona, alternating double filtration is more or less automatic in operation and requires only normal attention to the mechanical plant, the daily reversal of filter operation, and the withdrawal of sludge to the drying beds and raking of screens.

Cannery Wastes

Whilst it is desirable, where possible, for industrial effluents to be treated with domestic sewage there must obviously be many instances where public sewers are not available for the reception of the trade waste, especially in the processing of agricultural produce, which, for the greatest economy, is carried out close to the source of raw material.

The effluent from these processes consists of water used partly in washing and partly in the preparation for canning when qualities of organic matter are leached from the vegetables. This effluent is oxidized relatively easily and therefore the process of alternating double filtration should be eminently suitable for its purification.

With the recent growth of this industry, there has been established, at

Sharnbrook in Bedfordshire, a food-processing research establishment which is operated in conjunction with a large farm. The plant which has been installed is illustrated in *Figs 7* and *8* and is interesting on account of its flexibility. Although it has been designed to operate on the principle of double filtration it will be observed from the flow diagram (Figs 10, Plate 2) that it can be converted for experimental purposes to single filtration with or without chemical precipitation.

Since research and development were involved, no precise estimates of flow could be provided, but the works were designed to deal with a maximum quantity of effluent which might be anticipated at 40,000 gallons per day having a strength, measured by B.O.D., of 400 parts per million.

In view of the uncertainties of quantity and rate of flow, the first unit of the works consists of a holding-up tank having a capacity of 20,000 gallons, and this is provided with a coarse manually operated screen to prevent blockage by large solids. From this tank the liquid flows to a pump sump where provision is made for dilution either with effluent or lake water up to 100 per cent of the flow of waste, making a total pumping rate of 4,000 gallons per hour. The pumps installed are vertically arranged unchokable sewage pumps, a pair being installed for each suction sump (see *Fig. 9*).

The diluted liquid is delivered into sedimentation tanks which provide a total capacity of 4 hours' flow, the tanks being of the Dortmund type and in duplicate. From the Dortmund tanks the effluent is treated by the alternating double-filtration system, in which the liquid is passed through two percolating filters operating in series. Arrangements have been made to enable the order of treatment by the filters to be reversed, so that the primary filter on one day becomes the secondary filter on the next (see Figs 10, Plate 2).

The capacity of the filters is based on a rate of treatment of 160 gallons of settled and diluted crude sewage per cubic yard of media in the two filters per day, requiring a total filter capacity of 500 cubic yards, each filter being 6 feet deep.

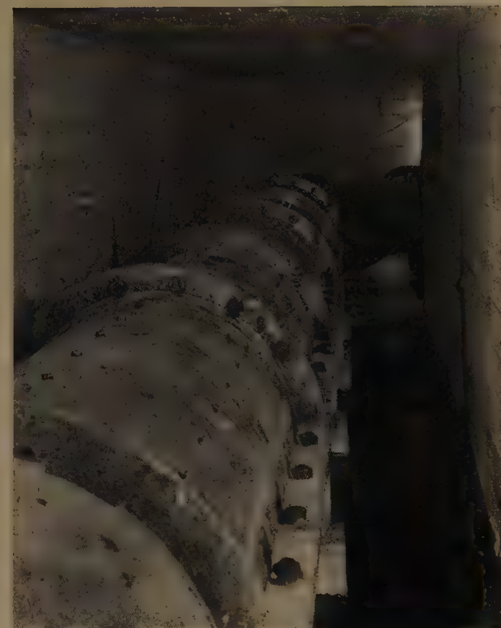
Following each stage of filtration the liquid passes through humus tanks having a total capacity of 6 hours' flow, one third of this capacity being used for settlement of the primary filter effluent and the remaining two thirds for the secondary effluent. Drying beds are provided for the sludge from the settlement tanks.

The effluent from the works is discharged into a very small stream which is dammed below the outfall pipe to form a lake, and here again the question of dilution is of moment. Since the works were put into operation in 1950, an excellent quality of effluent has been produced and there is no evidence of deterioration of the condition of the lake.

Fig. 9

Vertically Arranged Sewage pumps. Treatment Works at Sharnbrook

Fig. 12



Lead Lined Steel Sewer in Culvert. Cellophane Factory Effluent Sewer at Bridgwater

Fig. 13

Sewage Treatment Works, Cocking

Acid Effluents

Apart from the many problems inherent in the treatment of industrial and domestic wastes, one also encounters problems in the conveyance of effluents which may be of a corrosive nature and damaging to the normal constructional methods employed in sewerage.

Although the Public Health Act 1936 prohibits the discharge of corrosive liquids to sewers there are occasions when corrosive effluents have to be conveyed from point to point, for example, where an acid effluent has to be collected within a factory and conveyed to a preliminary treatment plant before discharge to a river or a public sewer.

A cellophane factory at Bridgwater discharges a hot waste with a temperature which is at times higher than 100° F. and containing up to 2 per cent of sulphuric acid, and although this liquid has to be transferred a distance of only 270 yards, the problem has created innumerable difficulties. Concrete, iron, and steel are all attacked by acid, and bitumen is adversely affected by high temperatures. Glazed stoneware pipes are satisfactory but the conventional jointing materials are also attacked by acids and, at the same time, they are very rigid and apt to fracture.

In this particular instance, where the foundation is not very sound and where vibration occurs from the railway line passing over the pipe, the sewer has been constructed in homogeneously lead-lined 27-inch-diameter mild-steel segments (see *Figs 11*) and placed in an open culvert so that it can be readily inspected (see *Fig. 12*). For the remainder of the length, 27-inch-diameter glazed stoneware is being used with joints of acid-resisting mortar. Since the River Parrett is tidal and contains mud in suspension which is alkaline and immediately neutralizes the acid, the effluent can be discharged without treatment.

The Rural Problem

The majority of the urban areas in Great Britain are adequately served by piped water-supplies and proper drainage facilities, but in many rural communities water is drawn from the well and earth closets are the rule.

The Rural Water Supplies and Sewerage Act of 1944, however, gave a fillip to work in rural areas by encouraging the provision of piped water-supplies and drainage schemes by means of Government and County Council grants.

This position has created two problems, the first being the difficulty of preparing and completing an enormous number of relatively small schemes in a short space of time. This problem was accentuated by the shortage of engineers with experience of the design or construction of this type of work owing to their preoccupation from 1939 with military engineering.

The second problem in the rural areas is a more technical one, namely, whether it is preferable and more economical to construct small separate

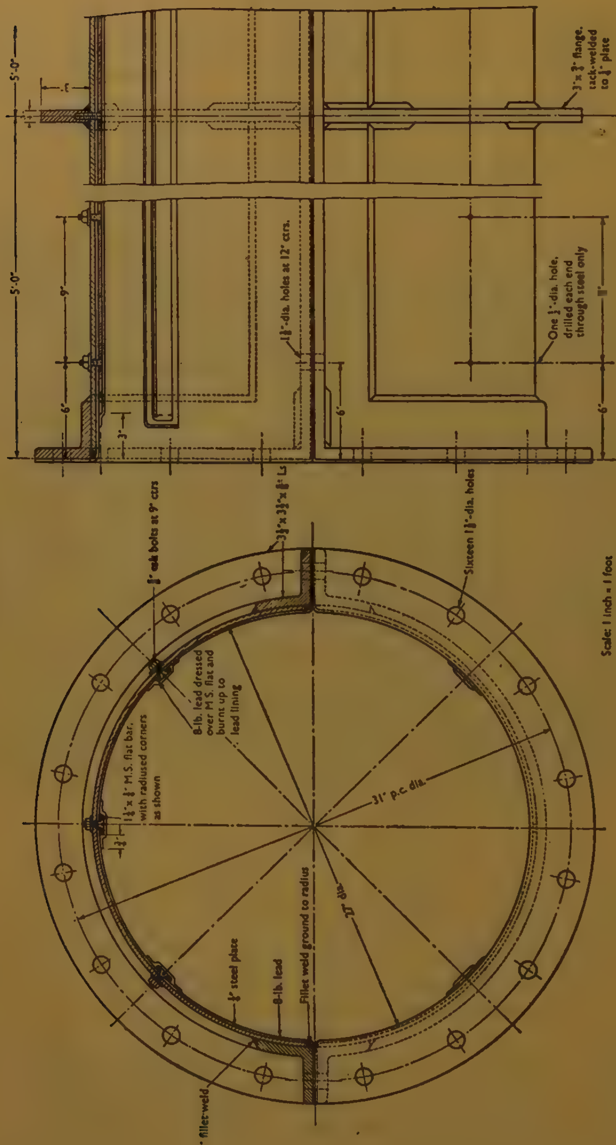


Fig. 11

DETAILS OF FABRICATED LEAD-LINED STEEL PIPE
CELLOPHANE FACTORY EFFLUENT SEWER AT BRIDGWATER

works for each village or to attempt to link up several villages by means of main trunk sewers to a central sewage-treatment works. The disadvantage of the first alternative is that it adds to maintenance cost in attending to a number of small works, but the second alternative involves much higher cost with the tendency for sewage to become septic while flowing long distances through small-diameter pipes or being held up for long periods in pumping mains.

Whilst each case naturally requires considerations on its merits, the Authors' view is that, in general, the former is the proper policy to adopt, and the problem of supervision of operation of works in rural areas can be overcome by the formation of mobile gangs with adequate equipment and experience, who can well be responsible for the operation of several works.

The design of small works for rural authorities presents no unusual problems and the Authors' consider sedimentation followed by percolating filters to be the most suitable process by virtue of its simplicity and robustness in operation.

Fig. 13 (facing p. 151) is a view of a typical works for a rural village near Midhurst, Sussex, and while in no way remarkable it does indicate that the work of the Public Health Engineer sometimes takes him into the pleasant countryside away from congested cities and industrial surroundings.

The Future

What are the problems of the future ? The most pressing problem is perhaps that of keeping pace with the growing demand for larger water-supplies for industrial and domestic purposes and the consequent requirement of new or larger sewage treatment works. This situation has been accentuated by the cessation of such works during the Second World War and by the subsequent national economic difficulties and labour shortages, which are both still very much in evidence.

A second problem is that of maintaining the purity of our rivers in the face of increased industrialization without putting too heavy a burden on the finances either of industry or of Local Authorities.

A third problem is the necessity for greater knowledge obtained from research work on the best methods of dealing with new developments such as the use of synthetic detergents and the manufacture of radio-active products. Research may also indicate methods of further extraction of valuable by-products from sewage and a solution of the problem of the economic return of sewage or sewage sludge to the land as a fertilizer, which assumes greater importance with the dwindling food resources of the world.

This Paper has touched on just a few of the many problems which have faced and are facing the public health engineer, but they are problems which must be solved if the purity of the waterways of the country are

to be maintained and the health and prosperity of the community ensured.

Finally the Authors would like to record their indebtedness to those Public Corporations, Local Authorities, and Companies and their officials—too numerous to mention individually—whose assistance and collaboration have made this Paper possible.

The Paper is accompanied by ten photographs and three sheets of drawings, from which half-tone page plate, folding Plates 1 and 2, and the Figures in the text have been prepared.

Discussion

Mr J. T. Calvert introduced the Paper with the aid of a series of lantern slides.

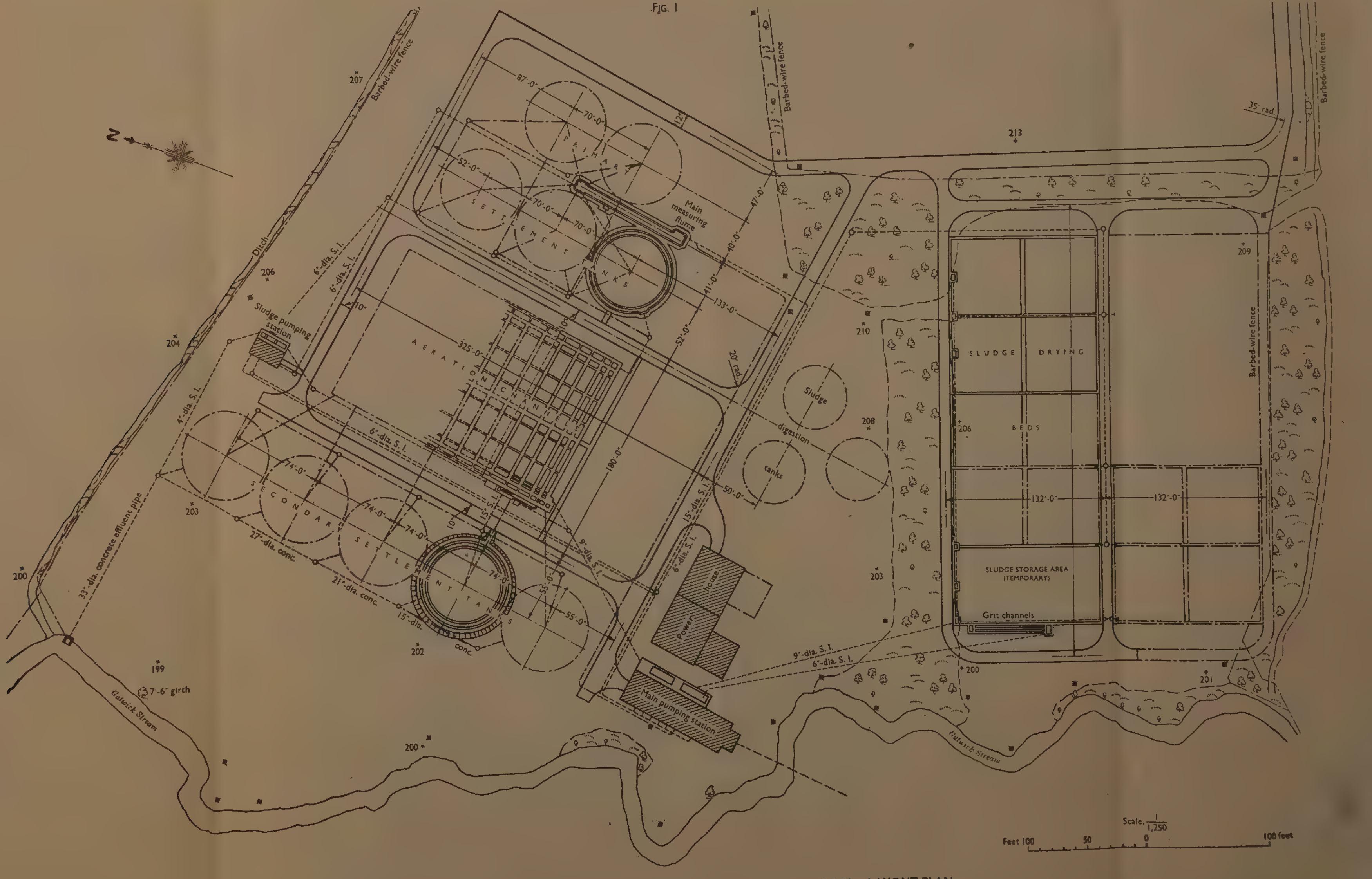
The Chairman said that the Paper was very readable, and so well written that, once taken up, it was impossible to put it down until it had been finished, and, after reflection the reader wanted to return to it and read parts of it again. In the historical section, reference had been made to the fact that many of the rivers of Great Britain had been seriously polluted in the past. A few days ago he had been reading some evidence given before the 1829 Royal Commission on the state of water in the Metropolis. In those days the water companies took their water from the river right in the middle of London, and one of them, probably the biggest offender, the Grand Junction Company, had an intake at the mouth of the Ranelagh sewer near the Chelsea Hospital. A doctor, who gave written evidence to the Commission, said he had visited the site and, the tide being low, had gone on to the causeway near the landing to the Chelsea Hospital to make a closer inspection, " when the foul brackish stream of the Ranelagh sewer passing between the steam engine and the dolphin "—the dolphin being the intake—" was seen to be loaded with no small portion of individual floating filth from the privies." Such was the description of the water, which in those days was pumped, without any filtration, to the water consumers of London. Although some of the rivers of Great Britain were in a dreadful state at the present time, they were not quite so visibly bad as that.

The Chairman would like to have one tilt at the Authors. At the foot of p. 145 they spoke of a flow of so many gallons per head per day for so many people, and by a process of multiplication it was possible to arrive at the amount of effluent which had to be dealt with. At the top of the next page they referred to cusecs, and a little further down the page they gave the capacity of the sludge tanks in cubic feet. Fortunately, they had avoided using acre-feet and tons per minute on the same page ! That was no reflection on the Paper, because it was something which anyone might

SOME PROBLEMS IN THE DISPOSAL OF INDUSTRIAL EFFLUENTS AND DOMESTIC WASTES

PLATE I
DISPOSAL OF INDUSTRIAL EFFLUENTS
AND DOMESTIC WASTES

FIG. 1



CRAWLEY DEVELOPMENT CORPORATION SEWAGE TREATMENT WORKS—LAYOUT PLAN

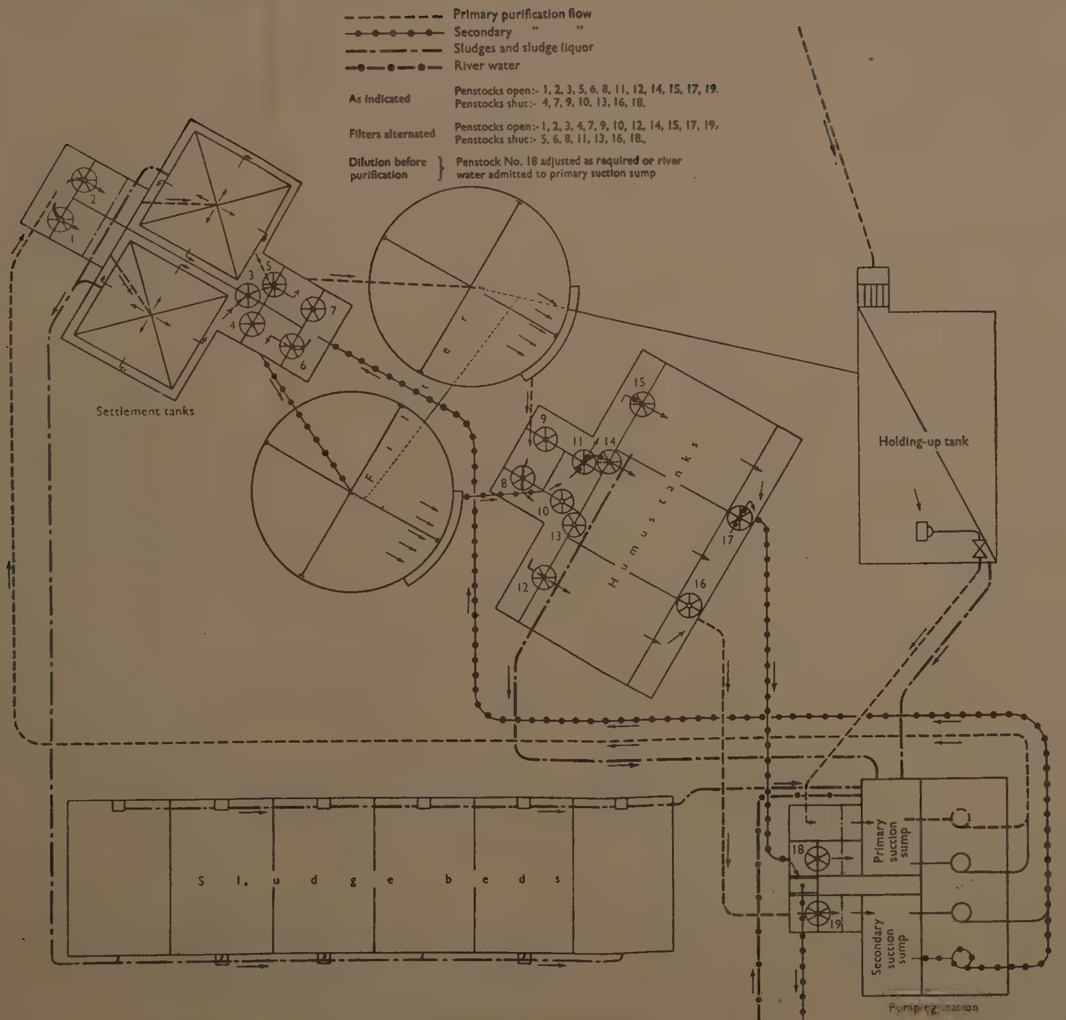
The Institution of Civil Engineers Proceedings, Part III, April 1952

J. T. CALVERT and P. M. AMCOTTS

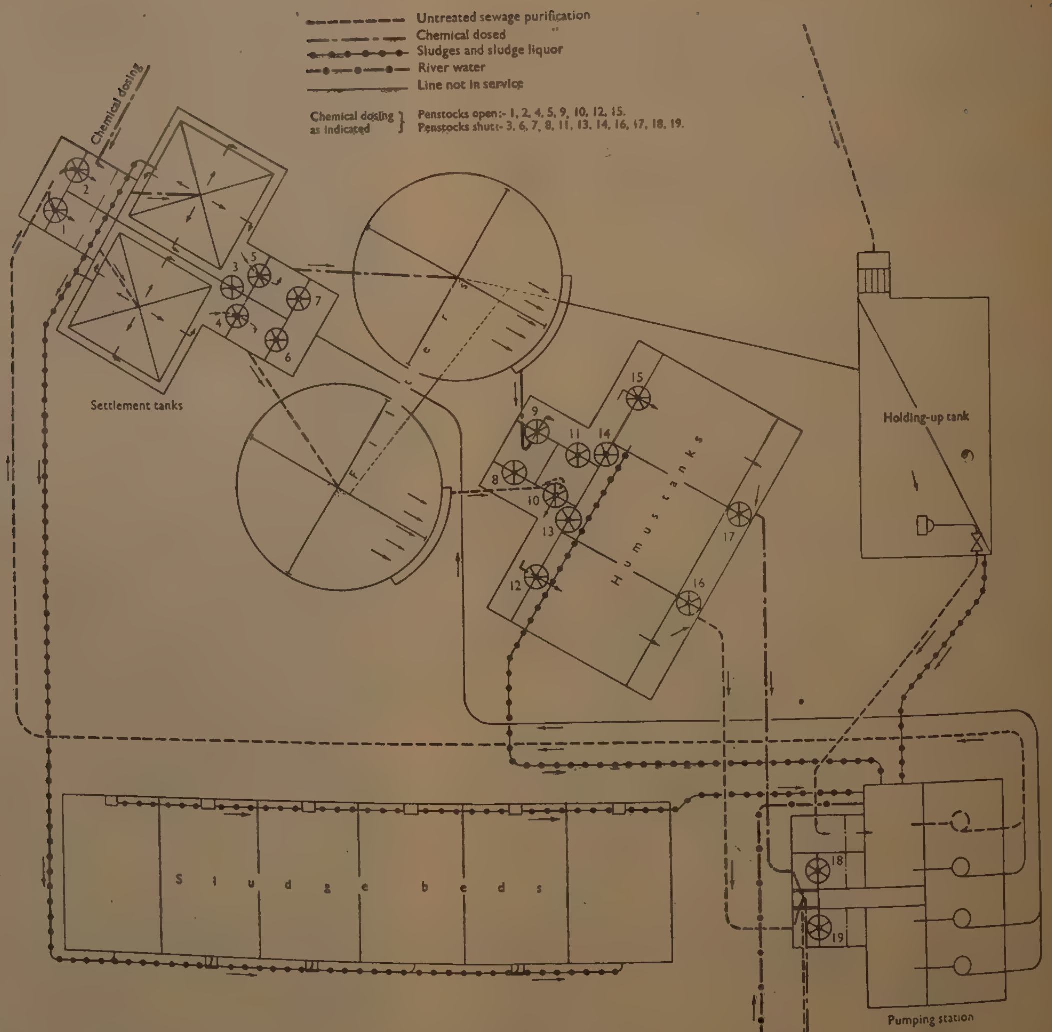
WILLIAM CLOWES & SONS, LIMITED: LONDON

SOME PROBLEMS IN THE DISPOSAL OF INDUSTRIAL EFFLUENTS AND DOMESTIC WASTES

Figs 10



ALTERNATING DOUBLE FILTRATION



PARALLEL OPERATION

LEVER BROTHERS, SHARNBROOK, TREATMENT WORKS OPERATION DIAGRAMS

do, but some people found it confusing to have to jump from one type of unit to another.

At the end of the Paper, reference was made to something which in due course the Division ought to consider, namely the economic return of sewage or sewage sludge to the land as fertilizer, because anything which would enable more food to be grown in this country was of the greatest national importance.

Mr F. C. Vokes said that the Birmingham Thame and Rea District Drainage Board had always consented to trade effluents being admitted to the sewers except in those cases where the sewage would thereby be rendered incapable of satisfactory treatment.

Experience at the Board's Yardley Works had been of particular interest. In 1924 the character of the sewage had been largely domestic. By the year 1936 the sewage was frequently acid and often rich in soluble salts of iron. On the filters brown slimy iron deposits impeded percolation and efficient aeration, and caused ponding. That had resulted in rapid deterioration of the effluent.

In 1938 extensions to the works had been brought into operation to enable them to deal with a dry weather flow of 6 million gallons per day. The extensions provided for liming and pre-aeration of the sewage, sedimentation in mechanically cleaned tanks which gave a longer period of retention, partial treatment of the tank effluent by bio-flocculation, completion of treatment on the existing filters and final settlement in tanks from which the humus sludge was removed continuously. As a result of the improved means of treatment, the acidity of the sewage had been corrected and the iron intercepted ; the condition of the filters had steadily improved, and the effluent obtained was satisfactory.

Referring to sludge disposal, the heated digestion tanks were fed with a uniform mixture of crude sludge, surplus activated sludge, and humus. The sludge digested well and the yield of gas was satisfactory.

Those conditions had continued until 1946 when frequent flushes of sewage arrived containing sulphuric acid and chromium wastes. The bio-aeration plant was unable successfully to treat sewage containing such concentrations. The condition of the activated sludge had deteriorated and, at times, the sludge rose and passed over the weirs of the separating tanks onto the filters. Since the undesirable trade wastes were particularly noticeable at week-ends, it had become the practice to by-pass the bio-aeration plant during those periods and pass the tank effluent direct to the filters.

With a view to improving the final effluent, re-circulation of the settled filter effluent was tried on an area of 1 acre of the filters. The experiments carried out over a period of 18 months had so far given disappointing results.

Referring to the effect of the trade wastes on sludge disposal, since 1946 the Works had been called upon to deal with a much greater quantity of

sludge of a type less amenable to treatment. In consequence, additional tanks had been constructed in which cold digestion could take place, but it would also be necessary to provide additional heated primary digestion tanks.

Mr Vokes said that the trade wastes to which he had referred were discharged into the Birmingham sewers. As the result of co-operation between the Corporation and the Board some improvement in the position had recently been observed, but a great change would be needed before the Yardley Works could operate as efficiently and economically as they had done throughout the War period, from 1939 to 1945.

Purification processes were dependent upon living organisms. It was not right that those organisms should be destroyed and valuable works put out of action by unduly concentrated flushes of trade wastes. One day industrialists would realize the losses they incurred by discharging valuable chemicals into the sewer, instead of economizing in their use or arranging to recover them after use.

Dr A. Parker agreed with much that the Authors had said about the treatment of sewage, water pollution, and trade effluents. Since Crawley, when fully developed, would still be a relatively small town, of 50,000 to 60,000 inhabitants, what had led to the selection of the activated-sludge process as against, say, the ordinary percolating-filter system or the alternating double-filtration system? No doubt it had been necessary to take into account site conditions and the relative overall costs—capital costs, costs of operation, power costs, and costs of supervision. It was generally acknowledged that the activated-sludge process required more skilled supervision than did percolating filters, and the question of the calibre of the staff and their pay would have to be taken into account.

At what stage did the Authors think that it paid to collect the gas and use it for generation of power? He had not calculated the power which could be produced, but probably with a population of 50,000–60,000 and the usual sludge and gas production, using small units of not very high efficiency for the production of power, it should be possible to generate approximately 60 kilowatts or 80 horse-power. That might mean employing extra staff, including engine drivers, who were highly paid, especially if they had to go on shift work, with the usual overtime rates.

He agreed with the Authors, Mr Vokes and the Chairman that the disposal of the sludge was still one of the unsolved problems of sewage treatment. On the other hand, it should not be imagined that sewage sludge was a very rich fertiliser. The usual experience was that it was not worth high transport charges to the farm. It might be a good soil conditioner in certain instances, but its fertilizing value as regards nitrogen, phosphorus or potash was low.

On the question of trade effluents, he agreed with what had been said about the Public Health (Drainage of Trade Premises) Act, because he considered that it encouraged the making of working agreements between

the sanitary authorities and the traders. He had had the privilege of being associated with Mr Wells, at the Ministry of Health, in the first two appeals under that Act in 1940. Probably people were inclined to work together as a rule without going to appeal, and sometimes appeals arose because of personal animosity rather than technical difficulties, but the trouble could be overcome in most instances. He would emphasize, however, that whatever agreement was made should be of a type to encourage the trader to recover as much material of value at his own works as was economically possible before discharging effluent to the sewers, because, after all, polluting material from a works was valuable material in the wrong place, and often the amount of polluting matter could be economically reduced and returned to the production unit.

Several trade effluents had been mentioned, and among them dairy wastes. If most dairies would look at their own operations they would find that they could cut down the waste going to the liquid effluent for disposal. The Water Pcllution Research organization had found, for example, that with pasteurizing plant, plant for washing out the milk churns, and so on, by systematic drainage—instead of just tipping up a churn and leaving a few quarts of milk in it—the amount of milk in the effluent could be cut down very considerably, by the equivalent of about 0·3 per cent of the milk treated. That represented a considerable saving of milk, which went to the sales tank instead of to the effluent, and it greatly reduced the load on the effluent treatment plant.

Single filtration did not work with milk effluent, which was too rich a food, and choked the filters. For dairy wastes the Authors had tried both alternating double filtration and "Aluminoferric." Dr Parker had had some experience of "Aluminoferric" and had found it very erratic, even if it were possible to adjust the dose of the chemical in proportion to the effluent flow, because much depended on the state of decomposition of the effluent when treated with the "Aluminoferric," and that depended not merely on the rate of flow in relation to the size of tank but also on the temperature at the particular time of the year. In any case he did not think that the average purification with "Aluminoferric" would be more than 50 per cent, whereas with percolating filters or activated-sludge treatment it was more than 90 per cent as measured by biological oxygen demand.

Gas liquor effluent presented a difficulty where the gas works served an area very much larger than that served by the sewage works, but one solution in many areas might be to regionalize sewage treatment. If the regions could be made to correspond roughly with the regionalization of gas manufacture, one difficulty would be largely overcome. There again the agreements, in his view, should be such as to encourage the gas industry to recover products of value—phenol and so on.

Reference had also been made to cannery wastes. There again, double or single filtration or similar processes could be used, and the same applied

to beet-sugar effluents. In treating vegetable wastes of that kind, where vegetables or sugar beet were being washed, it was not wise to have the primary sedimentation tanks too large. Sometimes people said "You recommend a certain size of sedimentation tank; we will double it and make sure." If that were done there would be solid matter in the sedimentation tank which would ferment and rise, causing further pollution of the liquor and increasing the problem of liquor treatment. The sedimentation tanks should be such as to bring down the major solid matter as rapidly as possible, and then that should be got away from the liquid; that had been his experience.

Each trade waste question had to be considered on its merits and in relation to the local conditions, whether there were local sewers or not, and so on. A certain experimental station dealt with foot-and-mouth disease. The animals were isolated, and anyone visiting the place had to strip, put on special clothing and walk into a bath of Lysol before entering the building, and into another on coming out, and before going back to the world outside he had to go under a shower of Lysol and then put on his own clothes.

In the particular case in question, something had to be done with the liquid effluent and with the more or less solid, semi-solid, or semi-liquid manure, and it was not an easy problem to solve. It had been more or less decided that it would be necessary to burn the solid or semi-solid matter and at the same time make sure that fine particles were not dispersed over the surrounding district.

In conclusion he would again emphasize that, so far as trade effluents were concerned, the trader should be encouraged to do everything he possibly could, economically, to recover as much as possible of the material discharged—he could not recover it all—and thereby cut down the cost of treatment. That could be encouraged if the right type of agreement was made.

Mr F. W. Roberts said that the Authors had referred to the effect of recent legislation, and in particular to the Rivers (Prevention of Pollution) Act, 1951, which empowered the newly-formed River Boards to set up standards; and the Royal Commission's standard would very largely be the basis on which such standards would be erected.

Methods had been elaborated for the measurement of pollution, but further progress was being made and biological determinations of the behaviour of plants and animals and organisms were going to be the testing ground for whether or not a river was polluted. The case of Luton had been mentioned. There the authority had had a legal action taken against them and were under the obligation to produce what was a "trout stream" from an undiluted sewage effluent. The standards which would be required for that purpose were not easy of definition by any chemist, and the health of the trout would be in fact the deciding factor as to whether or not the result was satisfactory.

To refer to more normal conditions, the Royal Commission laid down in their Recommendations a general standard of 30 parts per million of suspended solids, and 20 parts per million of biological oxygen demand. Of all the Royal Commission's Recommendations, that one had remained throughout the intervening years as their most publicized finding.

When the Royal Commission had made their Recommendation of eight times dilution they had admitted that, although they had gone all over Great Britain searching for examples on which to base their Recommendations, in no case had they found an authority which discharged a sewage effluent into less than eight times dilution. That was a very important fact, because there were many very large cities and towns which had by no means eight times dilution to-day. Luton was the extreme case, because there, for a long period, there was no dilution; but the authority which Mr Vokes represented would probably be hard put to it to find eight times dilution. Whilst the Royal Commission referred to what might happen below eight times dilution they had no experience of it, because, on their own admission, there were no cases of it at that time.

They recommended that the standard test for pollution requirements or effluent standards should be the biological oxygen demand test, which was a measure of the rate at which polluting matter took up oxygen from a river. The factor which countered that was the rate at which oxygen was being dissolved into the surface of the water of the stream. The Commission had carried out a number or experiments to show that the rate at which oxygen was dissolved in from the surface depended upon the depth and velocity of the stream, and that a fast-running stream 2 feet deep would dissolve oxygen 15 times as quickly as a stream 10 feet deep and slow-running. As the purpose of those tests was to ensure that the river maintained a satisfactory oxygen content, it was not only dilution but the physical condition of the stream which mattered, and the maintenance of a minimum of 60 per cent saturation of dissolved oxygen had been claimed by the Commissioners to be the requisite standard. That might well be a more important basis for a standard than just the paragraph which was so often quoted from the Royal Commission's Report.

Mr C. B. Townend observed that the Authors had given a brief sketch of the revolution which had occurred in sanitation and public health during the last 100 years or so, leading up to the position to-day, when there were numerous trustworthy tools with which to carry out the job of sewage purification. As so admirably illustrated by the practical examples given in the Paper, one of the most important factors in achieving success was the proper selection of processes to meet any particular problem.

If it were not for the national economic situation, the time would be ripe for a big step forward in the cleaning up of our rivers, as foreshadowed by the passing of the Rivers (Prevention of Pollution) Act, 1951. The purification of domestic sewage could now be carried out with certainty by a variety of well-tried processes, and there was no doubt that the largest

problem remaining was that of trade wastes. It was universally accepted to-day that the sewage works was the proper place for treating all suitable trade wastes, and that the pollution of rivers by industry could be most effectively prevented in that way. At the same time it had to be remembered that all modern methods of sewage treatment depended on biological processes. Those provided by far the cheapest method of dealing satisfactorily with waste matter of all kinds, wherever such treatment could be suitably applied. In point of fact the purification of average sewage could be carried out in that way at the extraordinarily small cost of about ½*d*. per ton of polluted liquid, although, of course, costs were rising to-day.

Whilst biological processes could be adapted to deal with wastes of a large variety, if they could be adequately diluted with the sewage flow, in the long run there must be a limit to the diet which living organisms could tolerate; and therefore there must always be some cases of effluents which, either because of their composition or because of their volume, could not be accepted by a sewage works without some modification by pre-treatment or otherwise. The solution of problems of that kind could almost invariably be found in one way or another, and in that connexion the work of the Water Pollution Research Board had been most valuable.

It was obviously in the interests of the whole community, and therefore of the traders themselves, that sewage works should be operated at the maximum possible efficiency and with the greatest economy; yet the operation of many works to-day, such as the Yardley Works of the Birmingham Drainage Board, was being very seriously impaired by the discharge of damaging trade effluents, and local authorities seemed to be wholly unable to remedy the position, because of the recognition of prescriptive and other rights under the Public Health (Drainage of Trade Premises) Act of 1937. It might well be that many authorities would be faced with a standard of purity for their effluents in the future, under the Act of 1951, with which they would be quite unable to comply on account of the rights given by Parliament to traders under the Act of 1937, and much of the benefit of the new legislation would be lost in that way.

It could never be right that the cost of sewage treatment for a whole town should be seriously increased, or that a vital public health service should be jeopardized and possibly put completely out of commission, because of the waste from perhaps one factory. It could not be right that the traders who had been getting rid of wastes for a number of years at the expense of the community, and in many cases causing harm to sewage works processes, should be able to go on doing so in perpetuity. What was considered the right thing for new traders to-day should be applied to all traders, and the 1937 Act would have to be amended accordingly before further progress could be made in cleaning up many of the rivers in Great Britain.

Mr H. D. Manning said that the Act of 1937 gave a very wide degree of freedom to traders who were discharging effluents prior to the passing

of the Act, giving them almost *carte blanche* within the limits of what they had done prior to 1937. To-day, 14 years after the passing of that Act, many towns were still taking no interest in the Act of 1937 and making no use of their powers under sections 9 and 10 of that Act to obtain particulars of the trade effluents which were discharged into their sewers. Many of them seemed to take the view that by inquiring into those trade effluent questions they would be upsetting the industrialist. That, of course, was quite a false idea; very frequently, by inquiring into the matter and finding out what was being done, it was possible to benefit not only the sewage works where the effluent had to be treated, but also the industrialist concerned, by investigating the absolute necessity for the effluent.

In "Wastes Engineering" for November 1951, reference was made to the recovery of vitamin B_{12} from sewage sludge, for the treatment of anaemia. That had struck him as an odd idea and as carrying the matter rather further than he had dared to do, but he would suggest that the old concept of sewage disposal was a little out of date, and that the time had come to think in terms of the treatment and reclamation of the liquid wastes of the community. There might be little hope of persuading people to talk about reclamation of liquid wastes when they meant sewage treatment, but it was at least possible to think of the matter in that way, because there were useful by-products that could be extracted from the material.

Mr C. Hogg referred to the section of the Paper dealing with dairy wastes, in which Authors had shown how a first examination of the local circumstances might result in substantial savings without undue risk. Some years ago an important firm of manufacturing chemists had been negotiating a new trade-waste agreement with their local authority, and, thinking to be on the safe side, they had asked for a sewer capacity of $1\frac{1}{2}$ times their peak flow. It transpired on inquiry, however, that the wastes were made up for the most part of continuous waters from continuous manufacturing processes and were substantially constant in volume throughout the 24 hours. A reduction from $1\frac{1}{2}$ to $1\frac{1}{4}$ times the average had been found permissible, and a saving of £6,000 resulted from that decision. In the same case, the polluting element of the wastes, though volumetrically unimportant, changed frequently from acidic to alkaline conditions and also occasionally contained organic solvents. The original sewer, in Portland cement concrete tubes, had collapsed after a life of only 3 to 4 years, and the use of chemically-resistant salt-glazed-ware pipes to B.S.1143 had at first been thought necessary. Samples of ordinary salt-glazed-ware pipes and of chemically-resistant pipes from the same maker had been tested, and, although the latter were naturally superior, it had been found that even the ordinary pipes complied with the stringent specification for chemically-resistant pipes. The ordinary pipes had therefore been used, at a saving of more than £15,000.

A weak point in such a sewer was the jointing material. In the case in

question it had been found that the only material suitable for the very variable conditions was a cold-setting synthetic resin, with a filler of barium sulphate. A technique for using that material under adverse field conditions had been worked out, but the material itself was very expensive. The pipe manufacturers, however, had co-operated by producing pipes with shallower sockets than standard. Since the pipes had had to be bedded and haunched with concrete for other reasons, the shallow sockets were no disadvantage, and substantial savings in jointing material had resulted.

The policy of dispersal of industry which had been pursued just before and during the war had brought into certain rural and semi-rural areas the problem of disposal of oil from engineering shops, engine test-beds, and the like. Cutting-oils or suds from machine shops had a high biological oxygen demand, of the order of 2,500 parts per million, and the admixture of those spent suds with sewage might result in the partial breakdown of the emulsion, causing trouble from floating oil in the sewers and at the sewage works. Although that might not be important in large industrial drainage areas, it might be very important in small sewage installations.

Again, at those factories the spillage of fuel oil and lubricating oil from drums stored in the open and washings from oily roadways, which had to be seen to be believed, would reach the surface-water system unless suitable precautions were taken. The effect and persistance of oil pollution of that sort might be judged from one case where a riparian owner 7 miles downstream had been able to obtain a permanent injunction in the Courts and had also been awarded substantial damages.

Mr Hogg showed slides featuring plant for treating oil before it was discharged into a local sewer. A special suds-collecting sewer brought the suds down to shallow flotation-channels, and any oil which would rise at that stage was allowed to do so. The remaining emulsion was then pumped into one of four 1,000-gallon batch-treatment tanks in a house, where treatment could be done in comfort and independent of the weather. The treatment was given by the addition of a coagulant after *pH* correction. An operator skimmed the scum from the top of the batch-treatment tanks. That scum, which of course was oily, could be burnt without difficulty on the works incinerator along with the ordinary rubbish from the factory, and the effluent was decanted and taken to the local sewer, having had its oil content reduced by about 90 per cent, and its biological oxygen demand reduced by half.

Mr P. Wedgwood said that a renewed attack was being made on the problem of the disposal of trade effluents, and in particular of gas liquors. There was much that could be done at most works to remove valuable materials from effluents. In the gas industry, phenol was one of those, and something more should be done to develop inexpensive methods for the recovery of phenols.

The Authors had stated that there had been controversy between local authorities and the gas industry, and unfortunately in many places that

was so and in some places it still persisted. He was sure that the Acts which had been passed in recent years would bring about a state of affairs where all those who had to dispose of awkward trade effluents would co-operate with the local authority, and the local authority would be very pleased to have that co-operation. More research work was necessary.

The question had been raised of the qualities, toxic or otherwise, of trade effluents. There was a section in the 1937 Act which referred to charges, and provided that the charges could be made having regard to the nature or composition of the effluent. That was going to be most difficult to decide, but he would suggest that, from a chemical point of view, as much or more was known about the nature of those effluents than was known about the nature or composition of domestic sewage. There was evidence that some of those so-called "toxic" constituents of effluents were more efficiently oxidised in a sewage works than some of the material which was present in domestic sewage. When more was definitely known about the composition of what went into a sewage works and what came out, and what were the end products—in particular the solid products, the humus—it would be possible to determine the best conditions in which those purifying reactions might be made to take place.

The lack of complete knowledge could on occasions lead to awkward conclusions. The Authors had said, in the section of the Paper dealing with Gas Liquor, that "there were indications of interference with biological purification as shown by the presence of thiocyanates in the effluent." Thiocyanate was present in domestic sewage, and might be expected to be there. He knew of sewage works where the treated effluent to-day contained thiocyanate, and there was no discharge from the gas works. With regard to what had been said about the treatment of dairy wastes, he was curious as to whether thiocyanate was present in the effluent from the sewage works in those cases. The concentration of thiocyanate which might be present there was very small, even where gas liquor was being treated, in comparison, say, with mother's milk. Some recent work had been done on that subject and a communication had been made to the Biochemical Society in October 1951. In his view, thiocyanate was a very good indication of the efficiency of the treatment of sewage in a sewage works, whether or not gas liquor was discharged to the sewers.

In connexion with the composition of humus, which was the final solid product, it had been found that complex aromatic hydrocarbons were present, which had been made in the process, and which, if they had been discovered by people lacking a full knowledge of the subject, would have been attributed to a gas-works discharge. He had in mind even such a substance as anthanthrene, traces of which had been found at a sewage works where there was no effluent from a gas works. The processes by which such substances were produced would be very interesting indeed. He thought that it was necessary to look into the matter further and find out just what the mechanism was by which those substances were produced.

Complex aromatic hydrocarbons were produced by synthesis from quite simple substances by bacterial action.

The solids from sewage works were not very good as manures. Methane, however, was produced by the digestion of humus, and it would be valuable to find out what were the best conditions for the production of gas by bacteria, in order to increase the yield of methane and so, perhaps, render sewage works self-supporting in power, and perhaps in the end even have surplus power for the grid system.

The Authors had referred to an experiment which it was proposed to carry out at Haverhill. That was a works which had not received gas-works effluent up to date, and it was hoped to carry out a series of tests before gas liquor was discharged, and then to discharge gas liquor in controlled and gradually increasing quantities over a period of time to determine the effect. That was part of a programme of work which was being carried out on a number of works by the Eastern Gas Board in co-operation with the local authorities, and he thought that eventually it would be useful as a contribution to the consideration of the best conditions for the efficient treatment of sewage containing trade wastes.

Mr J. W. S. Fawcett gave four examples of problems arising in connexion with industrial wastes. The first concerned the waste from the cellophane factory at Bridgwater, to which reference had been made in the Paper. He had been concerned with that problem before the factory was built, and had made several mistakes, the worst being to believe what he had been told would be the character of the effluent from the proposed works. He had been assured that it would be cold and would contain not more than 0·02 per cent of acid. After the works had been constructed it was found that the effluent was hot—the Authors had mentioned a temperature of 100° F.—which, of course, had had a disastrous effect on the bitumen lining of the steel pipes and concrete pipes which were used. At the time he had last been associated with it the acidity had been 0·2 per cent, and the Authors had stated that it was now 2 per cent. It had not been anticipated that those conditions would arise.

With regard to gas liquor, a short time ago the question had arisen of whether the sewage works should take the spent liquor from the gas works at St Helens. It was not a regional gas works, but the industrial load on the gas works was very heavy; the big Pilkington glassworks used a lot of gas. Had the whole of the flow from the gas works been taken, the proportion of gas liquor to domestic sewage would have been in the region of 1·3 per cent, whereas it was generally accepted that about 0·5 per cent should be the maximum. That percentage of 0·5 was not, however, a very good criterion, because it did not take into consideration the strength of the sewage or of the gas liquor. In the case in question it was decided, on the advice of analysts, that the oxygen-absorption figure of the sewage could be increased up to 160 parts per million by the addition of gas liquor, but even then only three-fifths of the gas liquor

could be taken, and the Gas Board had decided in the end that they would prefer to adopt what he believed was called the Tingley process of fractional distillation of the gas liquor, rather than pay for the very large extensions required at the sewage works. The aeration-tank capacity would have to be increased by 50 per cent.

At present he was concerned with the industrial wastes of two smallish towns in Lancashire. They were almost next door to each other, but the two problems were quite different. In one of the small towns the sole industry was tanning ; there were three tanneries in the town, and they all sent down a similar effluent, which was extremely upsetting to the sewage disposal works. A solution could probably be found by re-circulation and double filtration, but it would probably cost twice as much to deal with the mixture of sewage and tannery wastes as it would to deal with domestic sewage alone.

In the adjoining town there was again only one industry, namely the bleaching, dyeing, and printing of cotton. The flow from those works caused the *pH* of the domestic sewage to rise during the day to 9, 10 and even up to 12. The effect of that was to prevent any effective settlement in the primary tanks and to almost sterilize the sewage. At present he did not know the answer to that particular problem.

The solution to problems such as the four examples quoted lay in close co-operation between chemists and engineers.

Mr I. P. Haigh referring to the possibility of recovering chemicals from trade effluents, observed that much could be done in that direction, but from the point of view of the manufacturer the problems to-day were primarily those of high cost of construction, shortage of capital, and the very high level of taxation. Unless, therefore, it were clear to the manufacturer that he could get his money back within a very few years, he would probably be most reluctant to embark on any recovery scheme. As a result he tended, perhaps quite understandably, to regard money spent on recovery work as being money poured down the drain, and the engineer advising him on his new works was placed in a very difficult position between the local authority or the River Board on the one hand, and the manufacturer on the other.

It followed, therefore, that it was most urgent and important to establish by-laws or rules for the benefit of the manufacturer, so that he could judge in advance what his responsibilities were. He would probably be content to meet them if he knew what standards were expected.

With reference to those paragraphs of the Paper dealing with acid effluents, he admitted that he had been disappointed to find that so reasonable a solution had been possible in the case to which the Authors had referred. What would the Authors have done had there been no River Parrett in the immediate vicinity ? Neutralization with lime, whether in the form of limestone or by lime dosing, inevitably produced a high concentration of calcium sulphate. The solubility in terms of

calcium sulphate might be 180 parts in 100,000 at normal temperatures. What effect did that have on the materials of the sewers into which it was discharged? He believed that 180 parts of calcium sulphate corresponded to about 110 parts of sulphur trioxide, and that was a fairly high concentration, unless it were possible to ensure rapid further dilution within a short length of drain. In one case he had considered the possibility of using hydrochloric acid, as being an easier acid to neutralize, but there seemed to be very little knowledge or experience of what concentrations of calcium chloride salt it was possible to have in solution without injuring sewage treatment.

⁎ **Professor H. N. Walsh** said that he was particularly interested in the sections of the Paper dealing with the application of alternating double filtration to milk wastes and cannery wastes. In many dairies the amount of milk treated reached the maximum in summer and decreased to the minimum in winter. The difference in quantity of water used during the year was not so great as the variation in quantity of milk treated. In consequence, the waste to be treated varied in strength as well as in quantity, being weaker and less in quantity in winter, and the reverse in summer. It would be interesting to know if such variations had occurred at plants for which the Authors had designed alternating double filters and if any difficulty had been experienced in training the attendant to operate the plant so as to deal most effectively with such variations.

The suggestion, on p. 153, to use mobile gangs to operate works in rural areas was a good one. Groups of disposal plants serving small communities might well be serviced by even two men with a motor van to carry a portable sludge pump and other equipment.

The Authors, in reply, first apologized for mixing up the units used in the Paper. If the figure of 4 cusecs were replaced by 2 million gallons per day they felt the confusion would not be too great. They too had found the Reports of former Commissions (mentioned by the Chairman), extremely interesting as an indication of the conditions existing more than a hundred years ago.

It had been very interesting to hear Mr Vokes say so candidly that the re-circulation experiments which had been carried out at Birmingham had been disappointing, because the alternating double-filtration experiments there had been highly successful; it would therefore have been anticipated that, when it was decided to adopt re-circulation instead, that process would have been equally successful and more economical.

Mr Vokes, Dr Parker, and subsequent speakers had referred to the recovery of by-products, and the keynote of the whole discussion had perhaps been that an effort should be made to recover products from trade effluents wherever possible, and that recovery would often show a profit. It was, however, necessary to bear in mind that these trade effluents

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⁎ This contribution was submitted in writing.—SEC. I.C.E.

contained chemicals in very dilute solution, and the cost of recovery in such cases was high and might approach the value of the by-products.

Dr Parker had asked one or two questions about Crawley, and particularly about the choice of the activated-sludge process instead of percolating filters. Although economics had played a large part in deciding the selection of the process, perhaps the greatest factor had been the advantage of the activated-sludge process so far as the amenities of the district had been concerned. The Development Corporation at Crawley had been particularly anxious to have treatment works which would not involve any danger of a nuisance in the vicinity, and there was no doubt that percolating filters were prone to give more nuisance under certain conditions, both from smell and from flies.

Dr Parker had also asked about the time at which it was proposed to install sludge digestion. It was contemplated that this could be done economically when the population had increased to between 20,000 and 30,000.

Mr Roberts had raised the question of the Royal Commission's Report and the subject of dilution, to which reference had frequently been made. As Mr Roberts had said, the Royal Commission had found no case in which there was less than eight times dilution, and, if eight times dilution was to be a rigid figure below which a better standard of effluent was desirable, a great deal of work had still to be done to determine the river flow which should be used as the basis. The river flow varied a great deal between summer and winter, and between a normal summer and extreme drought, and if eight times dilution was to be treated as a boundary at which standards had to be altered it was unfair to adopt the absolute minimum flow for the purpose of calcluation of the dilution.

Both Mr Townend and Mr Manning had referred to prescriptive rights. There could be no doubt that if sewage treatment was to be carried out economically and the desired improvement in river quality was to be attained there would have to be some amendment of the law so far as prescriptive rights were concerned.

Mr Hogg's description of oil-removal plant was interesting. The Authors wondered whether Mr Hogg had tried the addition of compressed air in order to remove floating oil in the preliminary tanks. The Authors had seen that done, and it certainly assisted in bringing floating matter to the surface.

Mr Wedgwood had referred to the thiocyanates, and also to phenol removal. The Authors thought—though Mr Wedgwood knew more about it than they did—that the difficulty in the latter case arose because the phenol was in such small concentrations that its economic removal was problematical. Their experience was that the thiocyanates were not generally present in domestic sewage effluent from works which were functioning satisfactorily and not overloaded.

Mr Fawcett had referred to the Bridgwater factory, which had been

a source of great trouble. There was no doubt that it was difficult to obtain precise information from some manufacturers, especially in cases where they were not operating directly under the Public Health (Drainage of Trade Premises) Act, and did not have an agreement stipulating the maximum concentrations or temperatures of the effluent which the industrialist had to observe.

Mr Haigh had raised the question of acid neutralization, and his question really led to the much larger one of the location of industry. The Bridgwater site had been chosen because of the facilities for disposal of the acid. Before an industrialist established a factory which would create large volumes of difficult liquid he should consider the problem of its disposal when selecting a site.

In reply to Professor Walsh, the Authors confirmed his experience that in milk-processing factories it was customary to obtain larger volumes of a stronger waste in summer compared with winter flows. The treatment plant had to be designed for the worst conditions, of course, but one could also take into consideration the fact that percolating filters operated better in the summer by virtue of the higher prevailing temperatures. The Authors knew of no difficulty in training the operators to deal with such variations, and with alternating double filtration they would not recommend any modification to the method of operation between summer and winter.

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